

# ESSAYS IN APPLIED MICROECONOMICS

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# Preface

*“[...] economists – and everyone else – need two things to draw a conclusion: data, and some way of making sense of the data.”*

—LARS PETER HANSEN, 2019<sup>1</sup>

In recent decades the quantity and quality of data have increased rapidly and large administrative records or data from private firms present numerous new research opportunities (Einav and Levin, 2014). In the era of big data some argue that data speaks for itself. However, there are many steps that lead from raw data to conclusions. The more data available and the less structured it is, the more important a compass to navigate through it becomes.

The economist’s compass to navigate data comprises economic theory and econometric methods. Recently, statistical tools for the analysis of big data have also been brought into the field (e.g., Varian, 2014; Athey and Imbens, 2017; Mullainathan and Spiess, 2017). Typically, economists are interested in the causal relationship between two variables and the underlying mechanisms that drive the effect. Understanding these fundamentals is the basis for evidence-based recommendations for firms and policymakers.

By applying microeconomic methods to novel data, this dissertation aims to uncover causal relationships in two distinct fields: While the first three chapters consider different aspects of international migration, the last chapter investigates the effect of price transparency on the market power of firms.

Immigration is a highly contested issue, one which divides individuals within and between nations. A recent Gallup World Poll found that 15 percent of interviewed adults would like to move permanently to another country, if given the opportunity. Projected to the world population this would mean that about 750 million people have the intention to migrate.<sup>2</sup> The reasons for these intentions are manifold. On

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<sup>1</sup> 2013 Laureate of the Nobel Memorial Prize in Economics.

<sup>2</sup> Source: <https://news.gallup.com/poll/245255/750-million-worldwide-migrate.aspx>. Accessed 1 September 2019.

the one hand, there exist fundamental differences in living standards and population growth between countries (Hanson and McIntosh, 2016). On the other hand, there are certain shocks like conflicts, wars or natural disasters. Migration flows might become particularly large if fundamentally disadvantaged countries are hit by shocks.<sup>3</sup>

The decision to migrate does not only change the life of the migrants, it also has an impact on the people left behind and on those who live in the regions where the migrants settle. Migration thus concerns almost everyone in both the sending and receiving societies. While public discourse is often heated, it is not always based on evidence.

The first three chapters of this dissertation aim to contribute to the evidence by studying central questions concerning international migration. Chapter 1 investigates whether natural disasters trigger individual migration and what role existing migrant networks play for that decision. Chapter 2 analyzes how the economic conditions in the country of destination at arrival affect the socio-economic integration of immigrants. Chapter 3 studies how the distribution of public resources changes if undocumented migrants obtain legal status and eventually the right to vote. Gaining knowledge about these topics helps to predict future migrant flows and reap the benefits of migration.

The fourth chapter deals with a very different topic. It is motivated by the observation that many firms derive their market power, and thus their ability to price above the perfectly competitive level, from informational frictions. At the same time, due to modern information technologies, the costs for the collection and diffusion of information are very low. While in some markets private comparison platforms emerged, e.g. websites to compare prices for flights or insurance, in other markets policymakers mandate firms to disclose prices publicly. From a theoretical perspective, however, public price disclosure is not always beneficial for consumers. Slightly changing assumptions on how individuals search, the nature of goods or the elasticity of demand can have a great influence on the predictions of different theoretical models. The fourth chapter, thus investigates under what conditions mandatory price disclosure is likely to be pro- or anti-competitive.

A common theme of this dissertation is that either novel data and/or new identification strategies are used. Chapter 1 combines unique administrative data on 1.8 million Filipino permanent emigrants with geo-coded meteorological records to study migration responses to typhoons at a very local geographic level. In Chapter 2, long waiting times induced by the US immigration system are exploited to address the endogenous timing of immigration. In Chapter 3, a nation-wide amnesty program in the

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<sup>3</sup> A recent example is the Syrian civil war which lead to large refugee flows into Europe in late 2015 and 2016.



US that legalized about three million illegal immigrants is used as a natural experiment to study the response of politicians to a change in the electorate. In Chapter 4, for the first time, data on almost 20 million price notifications in the German retail gasoline market prior to the introduction of price transparency is utilized to implement a difference-in-differences strategy.

The remainder of this section provides an overview of each chapter of this dissertation. Each chapter is self-contained and can be read independently. A consolidated bibliography is presented at the end of the dissertation.

CHAPTER 1, studies the effect of typhoons on migration. Typhoons, like natural disasters in general, push millions of people into poverty every year. Whether this leads to more or less migration is theoretically ambiguous but of central interest for policy-makers in both sending and receiving countries, especially in light of climate change that increases the damages caused by typhoons owing to rising sea temperatures. I combine individual-level data for 1.8 million Filipino permanent emigrants with meteorological records of typhoons and find that typhoons on average increase migration from exposed municipalities. However, this does not hold for all municipalities. The evidence suggests that only individuals in municipalities with sufficiently large migrant networks leave the country in response to typhoons. A key contribution of this chapter is to shed light on two important mechanisms that drive the network effect - providing legal entry and economic support. To investigate these mechanisms, I analyze migration to different destination countries and exploit variation in exogenous destination country factors. In particular, I present evidence that networks are more effective if family reunification policies of these countries are less restrictive and labor migration policies more restrictive. Furthermore, I show that the network effect increases if the network is economically more successful.

CHAPTER 2, which is joint work with Toman Barsbai and Andreas Steinmayr, investigates how economic conditions at the time of arrival affect the socio-economic integration of family migrants in the US. Even though family migrants account for 55 percent of all permanent immigrant arrivals in the OECD, they have received only limited attention in economics. This chapter builds on and contributes to the literature on how entering the labor market in a recession affects labor market outcomes. For identification, we exploit years-long waiting times for visas that make it impossible for family migrants to base their migration decision on economic conditions at the time of arrival. We find that unlucky migrants that arrive during worse economic conditions face considerable consequences. A one percentage point higher initial unemployment rate decreases wage earnings by four percent in the short run and by 2.5 percent in the longer run. This is due to substantial occupational downgrading. Family migrants who

immigrate during a recession move into lower-paying occupations with strong ethnic networks. Furthermore, they are more likely to reside with family members, which reduces their geographical mobility.

CHAPTER 3, which is joint work with Navid Sabet, sheds light on the question of what happens to the distribution of public resources when undocumented migrants obtain legal status, and eventually citizenship and the right to vote. To answer this, we exploit variation in the legal status of about three million illegal immigrants in the US through a nation-wide amnesty program, the 1986 Immigration Reform and Control Act (IRCA). First, we find that state governors allocate more per capita aid to those counties affected by the IRCA. Second, we posit that this is borne out by rational, forward-looking governors who allocate resources strategically in pursuit of the votes of the newly legalized who were eligible to vote five years after legalization. We provide evidence to support this view: We find that the distribution of state aid differs significantly according to political context and that counties affected by the IRCA receive more resources when their governor is eligible for re-election, faces political competition or enjoys line-item veto power. Third, our results indicate that the transfers were targeted to the newly legalized, mostly of Hispanic origin, and not other constituents. We also find that high school completion rates among Hispanics improved in counties that received more transfers from the governor. These findings contribute to the literature that sheds light on the distributional effects of the expansion of voter franchise as well as the literature on the economics of legal status.

CHAPTER 4, which is joint work with Felix Montag, asks under what conditions mandatory price disclosure is likely to be pro- or anti-competitive. The chapter makes three main contributions. Firstly, we introduce a theoretical search model with collusion and imperfectly informed consumers and producers. The model shows that the level of ex ante consumer and producer transparency, as well as the number of consumers adopting the mandatory price information, determine whether the policy is pro- or anti-competitive. Secondly, we extend the empirical literature on the effect of mandatory price disclosure on prices. In particular, we construct a unique data set to study the effect of mandatory price disclosure in the German petrol market. Thirdly, we investigate how different market conditions lead to differential effects of mandatory price disclosure empirically and use these results to assess the suitability of different theoretical models. We find that, due to the introduction of price transparency, retail margins decreased by 13 percent. We also find that the theoretical model is better at explaining the relationship between the treatment effect and ex ante consumer transparency than other models in the literature. Our results inform policymakers by highlighting how the effect of mandatory price disclosure depends on certain market characteristics.

## Chapter 1

# Natural Disasters, Networks, and Migration

### Abstract

I use administrative emigration data from the Philippines from 1988 to 2015 as well as exogenous variation in typhoon exposure to provide evidence on the effect of typhoons on migration and the specific role migrant networks play. A key contribution of this paper is to shed light on two important mechanisms that drive the network effect - providing legal entry and economic support. I find that a one standard deviation increase in typhoon exposure increases the migration rate of a municipality by about one individual per 100,000 or two percent of the mean migration rate. This effect is driven by individuals in municipalities with large migrant networks. Furthermore, networks are more effective if family reunification policies of destination countries are less restrictive and networks are economically more successful, i.e., if providing legal entry and economic support is easier.

## 1.1 Introduction

Recent estimates from the World Bank suggest that natural disasters cause about 300 billion USD in economic losses every year and push more than 26 million people into poverty.<sup>1</sup> According to the Centre for Research on the Epidemiology of Disasters almost half of all recorded losses due to natural disasters are caused by heavy storms (CRED, 2017). The destructiveness of these storms is highly correlated with tropical sea surface temperatures (Emanuel, 2005) and will thus increase as climate change progresses.

There are many different strategies individuals may adopt to deal with negative shocks such like natural disasters. Among the most extreme is to leave your home permanently. Understanding the effect of natural disasters on international migration is thus of central interest for policymakers in sending as well as receiving countries. For the country of origin, (selective) migration can increase inequalities between regions in times when affected areas are most vulnerable. For receiving countries, migration acts as a defacto channel through which they are indirectly affected by natural disasters. Since immigration is a very contested issue, it is crucial for receiving countries to be able to both better predict above average migration flows and better understand the selection process.

In this paper, I analyze the migration response to typhoons and the specific role networks in the country of destination play. First, I investigate whether typhoons in the country of origin cause migration in exposed municipalities and whether there is a selection of migrants with respect to gender, age, and education. Second, I highlight the important role that networks play in coping with natural disasters. Third, I shed light on two key mechanisms through which networks support migrants: by providing legal access and economic support.

The analysis is based on a panel of 1624 municipalities in the Philippines for the years 2001 to 2015. I estimate a fixed-effects model exploiting exogenous variation in typhoon exposure within municipalities over time as well as cross-sectional variation in network size. To investigate the mechanisms, I use variation in immigration policies and GDP per capita growth of destination countries.

Typhoons lower the attractiveness of remaining in one's country of origin. They destroy buildings and capital goods (Albala-Bertrand, 1993), cause business interruptions in firms (Leiter et al., 2009), unemployment, and negatively affect firms in the supply chain (Rose, 2004). Thus, typhoons depress the income of affected individuals (Anttila-Hughes and Hsiang, 2013). Since typhoons both decrease wealth and depress income, the effect of typhoons on migration is theoretically ambiguous. For some indi-

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<sup>1</sup> The World Bank - Results Brief - Climate Insurance (2017). Available at: <https://www.world-bank.org/en/results/2017/12/01/climate-insurance>. Accessed 10 June 2019.

viduals, liquidity constraints force them to remain in the country, while others migrate because of worse economic perspectives.

The role of networks in coping with natural disasters can also lead to both more or less migration. It has been shown that networks lower the costs of migration by providing legal access or help in finding a job in the country of destination (e.g., Massey, 1988; Munshi, 2003; McKenzie and Rapoport, 2010; Comola and Mendola, 2015). It might thus be easier for individuals with a existing network to migrate than for those without if hit by a typhoon. However, remittances provided by the network might help to improve the situation in the aftermath of a typhoon and thus lower the intention to migrate (Yang and Choi, 2007; Licuanan et al., 2015).<sup>2</sup> It is thus an empirical question and depends on destination country factors whether networks amplify or mitigate the response to typhoons.

There are different mechanisms through which networks support migrants. Providing legal access to enter a country of destination is one key factor put forward by the literature (e.g., Ortega and Peri, 2012; Mahajan and Yang, 2019). In this case, immigration policies of potential destination countries play a key role. To permanently enter a country, there are two main routes: Individuals migrate either through the family-based or the employment-based channel. If immigration policies are more restrictive with respect to family reunification it is more difficult for previous migrants to sponsor family members and thus the network effect should be less pronounced. If, on the other hand, employment-based immigration is more restricted, individuals have to rely more on their network and thus the network effect should be more pronounced.

Another mechanism through which networks influence migration is to provide economic support in the country of destination. However, the extent to which individuals in the network are able to do so depends on their own economic success. It is, for example, easier for individuals to arrange a job for others if they themselves are employed. I thus expect that the network effect is more pronounced if the network is economically more successful.

A main contribution of this paper is related to data. The Philippines is an ideal country to study this relationship since it is one of the largest migrant-sending and, at the same time, the most typhoon exposed countries in the world. I am the first to use administrative individual-level data of the universe of permanent emigrants from the Philippines from 1988 to 2015 and exploit exogenous variation in typhoon exposure at the municipality level to investigate the effect of typhoons on migration. The administrative migration data allows me to address a key challenge of the literature. Either

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<sup>2</sup> Gröger and Zylberberg (2016) investigate internal migration responses to a typhoon in Vietnam. They show that households with existing networks receive more remittances and that individuals in households without a network tend to migrate and then remit similar amounts.

researchers have to rely on administrative census data and only observe individuals every decade, or they have to rely on survey data with limitations regarding measurement and sample size. Furthermore, the data allows me to construct a measure for network size at the municipality level. This level of detail is generally not collected in the censuses of destination countries, which often only record the country of birth. I use migration flows from 1988 to 2000 as a proxy for a municipality's network.

To measure typhoon exposure, I use meteorological records that contain best track typhoon data, i.e., the center of every typhoon in 6h intervals. On the basis of key parameters of each typhoon and by applying a physical model proposed by Holland (1980), I am able to estimate the impact of every typhoon on the grid-cell level.<sup>3</sup> I then use grid-cell level population data to weight the impact by the number of affected persons and aggregate typhoon exposure at the municipality-year level.

For the analysis of the mechanisms, I rely on two different data sources. To study the role of providing legal access, I use data from the Immigration Policies in Comparison (IMPIC) dataset (Helbling et al., 2017). It contains several indices for the restrictiveness of immigration policies for all main destination countries. To investigate the role of providing economic support, I use data on past GDP per capita growth provided by the World Bank. Since I only observe migrants when they leave the Philippines and not in the country of destination, GDP per capita growth in the destination serves as a proxy for the economic success of the network. To support this approach, I provide evidence that past GDP per capita growth increases wage income and employment for Filipino immigrants in the US using the 2000 US Census and the 2001 to 2015 American Community Surveys.

I find that a one standard deviation increase in typhoon exposure increases the migration rate of a municipality by about one individual per 100,000 or two percent of the mean migration rate. However, the average effect hides significant heterogeneity. First, young children and spouses of previous migrants seem to respond particularly strongly to typhoons. The effects are also larger for less-educated individuals. Second, only individuals in municipalities with a sufficiently large network react. This effect is not driven by differences in population or income of the municipalities. Third, the facilitating role of networks is more pronounced if family reunification policies are less restrictive. Furthermore, it seems that networks are especially supportive if labor migration is more restricted, which indicates a substitution between relying on family reunification and migration through employment-based channels. I also provide evidence that the network effect increases if the network is economically successful.

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<sup>3</sup> Parameters that enter the model are, among others, the distance to the typhoon, ambient and central pressure, latitude information, the maximum wind speed of the typhoon, and the radius of the maximum wind.

When interpreting the results, a few things have to be kept in mind. First, because of the high frequency of typhoons that hit the Philippines, individuals likely incorporate the risk. The estimated effects thus have to be seen despite all measures in place to mitigate the impact of typhoons (Anttila-Hughes and Hsiang, 2013). Second, I focus on permanent migration, which means that I am not able to observe any short-term labor migration responses caused by typhoons. Third, since the identifying variation comes from typhoon exposure at the municipality level, the estimated effects represent migration responses at the local level in addition to macro-level responses.

I contribute to a fast-growing literature on the effects of climate change and natural disasters on international migration.<sup>4</sup> For example, Cattaneo and Peri (2016) look at the period from 1960 to 2000 and find that higher temperatures in poor countries decrease migration while they increase migration in middle-income countries. However, they find that the occurrence of natural disasters does not affect the migration rate. Beine and Parsons (2017) look at the same period and exploit bilateral migration flows. They find that natural disasters deter individuals from migrating on average, but spur emigration to neighboring countries and former colonies. Beine and Parsons (2017) explicitly mention that one reason for this finding might be established diasporas.<sup>5</sup> I take this idea to the data and investigate whether the response to natural disasters indeed depends on the size of the network.

I thereby build on the broad literature that studies the role of migrant networks. The literature has shown that networks play a key role in migration dynamics as well as migrant selection.<sup>6</sup> While in many studies, the network is modeled linearly, Chay and Munshi (2013) show that for black migration from southern US counties to northern cities in the early 20th century, network effects come into play only after a certain threshold of connectedness within a community. Using municipality-level variation allows me to investigate whether this also holds in the present setting. Munshi (2003) studies labor market outcomes of Mexican immigrants in the US and finds that the larger the migration network, the higher the likelihood of a migrant of being employed and receiving a higher wage. I contribute to this literature by investigating the specific role of networks in coping with natural disasters. Furthermore, I discuss how important the economic success of a network is for the network effect.

Drabo and Mbaye (2015) were the first to investigate whether natural disasters induce a brain drain, i.e., whether mostly well-educated individuals migrate. They

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<sup>4</sup> See Millock (2015), Berlemann and Steinhardt (2017) and Beine and Jeusette (2018) for recent surveys of the literature.

<sup>5</sup> Diaspora refers to the stock of people born in a country and living in another one.

<sup>6</sup> Among others Munshi (2003), Beine et al. (2011), Bertoli and Fernández-Huertas Moraga (2015), Patel and Vella (2013), and Docquier et al. (2014).

use bilateral migration data from developing countries to the OECD countries and show that natural disasters increase migration rates on average and more so for highly educated individuals. To extend their analysis and investigate whether networks only assist specific groups of individuals, I build on the literature on networks and self-selection (e.g., Carrington et al., 1996; Winters et al., 2001; Pedersen et al., 2008; McKenzie and Rapoport, 2010; Bertoli and Fernández-Huertas Moraga, 2015). In particular, I provide evidence on the heterogeneity of the response to typhoons with respect to education, age, and gender.

The paper most closely related to this study is Mahajan and Yang (2019). They use country-level data and find that hurricanes have a positive effect on migration to the US. Furthermore, they show that the effect size increases in a country's prior migrant network size in the US. The present paper complements the evidence provided by Mahajan and Yang (2019) in different ways.

First, instead of focusing on a country-wide analysis, I measure migration responses at the municipality level, which allows me to consider the question from a different perspective. In large countries, typhoons only hit certain areas directly. Thus, estimated effects using country-level data only reveal the average response of all areas, whether affected or not. It may well be that in directly affected areas migration decreases because of the adverse effects on wealth and in non affected areas migration increases because of, for example, updated beliefs about future typhoon exposure. In general, studies using country-level variation might thus conclude that there is no effect. In contrast, municipality-level data can reveal such a pattern.

Second, Mahajan and Yang (2019) provide evidence that, for migration to the US, providing a legal route of entry is a key mechanism through which networks operate. I build on this idea and extend the analysis to many destination countries. I use variation in the restrictiveness of immigration policies concerning family reunification and labor migration to investigate this mechanism.

The remainder of this paper is structured as follows: Section 1.2 presents background information on migration and typhoons in the Philippines. Section 1.3 introduces the data. Section 1.4 presents the empirical analysis. Section 1.5 discusses two key mechanisms and Section 1.6 concludes.

## 1.2 Background

### 1.2.1 Philippine Migration

The Philippines has a long tradition of migration and is one of the largest migrant-sending countries in the world (OECD, 2018). It is also among the ten countries with



the largest diaspora population (IOM, 2017). The migration system in the Philippines is highly institutionalized, and there are two channels through which Filipinos can emigrate. The first option is to leave as a temporary overseas Filipino worker (OFW) and register with the Philippine Overseas Employment Administration (POEA). To do so, individuals must have a legitimate work contract which is mainly organized by licensed private recruitment agencies (McKenzie et al., 2014). The other option to leave the country is to have a permanent visa (either employment-based or family-sponsored) and register at the Commission on Filipinos Overseas (CFO).

Data provided by CFO show that as of December 2013, about 10 million Filipinos lived abroad. Half of these Filipinos were temporary labor migrants and the other half permanent migrants. The destination countries of temporary and permanent migrants are very distinct. While temporary migrants predominantly go to other Asian countries or countries in the Middle East, the five most important destination countries for permanent migrants are the United States, Canada, Japan, Australia, and Italy. For these countries, the share of Filipino permanent migrants among all Filipino migrants is more than 90 percent.<sup>7</sup>

By law, every Filipino holding an immigrant visa, i.e., every permanent migrant, must register with CFO before departure. Otherwise, departing from the Philippines is not possible. At the time of registration with CFO the migration decision is already taken.<sup>8</sup> To register, a migrant must report to CFO in person, attend a pre-departure orientation seminar, present her immigration visa, and complete a registration form. After that, the migrant is allowed to leave the country permanently. The Philippines thus offers a unique opportunity to study migration, since it keeps track of its emigrants over decades. While this data has not been yet available for researchers, I have access to the universe of permanent Filipino emigrants from 1988 to 2015.<sup>9</sup>

Because I do not have access to data regarding the temporary migrants provided by POEA, I focus on permanent migration. Even though it would be interesting also to analyze the POEA data, by definition, temporary migrants return to their home country and thus flow data for temporary migrants might only reveal short term effects. In contrast, for permanent migrants, return migration is less of a concern.

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<sup>7</sup> CFO migrant stock data available at <https://cfo.gov.ph/news/34-statistics/30-stock-estimates-of-overseas-filipinos.html>. Accessed 25 February 2019.

<sup>8</sup> They also have only limited possibilities to postpone migration. For the US, for example, the visa is only valid for six months.

<sup>9</sup> I describe the data in more detail in Section 1.3.

### 1.2.2 Typhoon Climate in the Philippines

Typhoons are a highly destructive form of storms that fall under the category of cyclones, i.e., areas of low atmospheric pressure, characterized by rotating winds. They are accompanied by storm surges, strong winds, and flooding and cause substantial damage (Yang, 2008). The Philippines is one of the countries most hit by typhoons. On average about eight typhoons make landfall in the Philippines each year. Since the Philippines is a large country, various areas are affected differentially each year (Anttila-Hughes and Hsiang, 2013). Figure A.1 depicts the average typhoon occurrences per month. The main typhoon season is May to November.

To illustrate the destructiveness that typhoons can cause, I briefly describe the impact of typhoon Haiyan (also known as typhoon Yolanda), which was one of the strongest typhoons that made landfall in the Philippines in recent years. Figure A.2 shows a satellite photo of typhoon Haiyan and the route it took. Typhoon Haiyan entered the Philippines on November 7th, 2013, hit the city Guiuan in the province Eastern Samar and made its way eastwards through the Philippines. It affected more than 16 million persons in almost 600 municipalities (40% of all municipalities) with 6,300 confirmed casualties. The Philippine government estimated total damages worth billions of USD with substantial damage to the infrastructure (roads, airports, telecommunication, etc.), and severe disruptions to both the social sector (education, health, and housing) and the productive sector (agriculture, fishing, industry, etc.).<sup>10</sup>

## 1.3 Data

### 1.3.1 Migration and Census Data

The analysis mainly builds on administrative data collected by the Commission on Filipinos Overseas (CFO). The data covers the universe of permanent migrants and in total includes more than 1.8 million individuals. I have access to all individual-level registration data for the period from 1988 to 2015 (the period for which electronic records are available). The data contains basic personal information like gender, age and education level. It also states to which migrant category a person belongs, e.g., whether the person is an employment migrant or a spouse, child, sibling or parent of a former Filipino emigrant.

A key advantage of this data is that it contains information about the municipality of origin. This data is typically not available in the Census of the destination

<sup>10</sup>Philippine National Disaster Risk Reduction and Management Council report available at [http://www.ndrrmc.gov.ph/attachments/article/1329/FINAL\\_REPORT\\_re\\_Effects\\_of\\_Typhoon\\_YOLANDA\\_HAIYAN\\_06-09NOV2013.pdf](http://www.ndrrmc.gov.ph/attachments/article/1329/FINAL_REPORT_re_Effects_of_Typhoon_YOLANDA_HAIYAN_06-09NOV2013.pdf). Accessed 1 August 2019.

countries and surveys that include information about migrants at the very local level often face measurement problems. For the aggregation at municipality-level, ideally one would like to observe the municipality the migrant lived in when hit by a typhoon. While this exact information is not available, CFO data contains a migrant's current municipality at the time of registration at CFO, i.e., the last residence in the Philippines before migrating, and her municipality of birth. I use the migrant's municipality of birth for the main analysis and present robustness checks using the current municipality. This is due to the fact that the attenuation bias generated by internal moves during the lifetime is less severe than the negative bias potentially caused by internal migration before departure in response to typhoons (Schwandt and von Wachter, 2019).<sup>11</sup>

I supplement the migration data with information from the Philippine Censuses of 1990, 1995, 2000 and 2010 obtained via IPUMS (Sobek et al., 2015) as well as the 2015 Census obtained from the Philippine Statistical Office.<sup>12</sup> I use this information to calculate basic statistics like population and education level at the municipality level.

The main dependent variable is the migration rate  $y_{m,t}$  of a certain municipality  $m$  in a given year  $t$ , i.e., the number of migrants  $M_{m,t}$  divided by the population  $pop_{m,t}$  as shown in Equation 1.1. The migration rate is measured by one over 100,000 individuals, i.e., 1/1000 percent. For the heterogeneity analysis, the migration rate is also calculated separately by certain personal characteristics, destination countries, and migrant categories.

$$y_{m,t} = \frac{M_{m,t}}{pop_{m,t}} \quad (1.1)$$

Panel (a) of Figure A.3 shows the distribution of the yearly migration rate at the municipality-level between 2001 and 2015. It ranges from zero to about 1,000 per 100,000 and is on average about 60 per 100,000. Figure A.4 shows a map of the Philippines with the migration rate per municipality in 2015. Migration is most pronounced in municipalities at the west coast of Luzon Island.

Table A.1 summarizes basic characteristics of the population and the migrants for the full period (1988 to 2015) and for the years 2001 to 2015 separately.<sup>13</sup> Compared to the population in general, migrants are on average about ten years older and the share of migrants with at least secondary education is twice as high. Furthermore, two-thirds of migrants are female.

One of the main explanatory variables in the empirical analysis is the network size of a municipality which I measure as a share of municipality population. Specific-

<sup>11</sup>In any case, if I find a positive effect, the bias works against me.

<sup>12</sup>Population data from the 2015 Census available is at: <https://psa.gov.ph/content/highlights-philippine-population-2015-census-population>. Accessed 25 January 2019.

<sup>13</sup>I use data from 1988 to 2000 to calculate a proxy for the network size of a municipality and data from 2001 to 2015 for the analysis of the effect of typhoons on migration.

ally, I use administrative data provided by CFO to calculate the cumulative number of migrants who left a specific municipality from 1988 to 2000. I then divide that number by the population of the municipality in 2000 to obtain a population-adjusted measure of migration networks  $N_{m,2000}$  as shown in Equation 1.2. For the analysis of the destination country factors, I also calculate the network size destination country-specific.

$$N_{m,2000} = \frac{\sum_{1988}^{2000} M_{m,t}}{pop_{m,2000}} \quad (1.2)$$

The underlying assumption is that the flows between 1988 and 2000 are a good proxy for the actual network size of a municipality.<sup>14</sup> While the yearly migration rate is measured in 1/1000 percent, the network size in 2000 is displayed in percent. Panel (b) of Figure A.3 shows the distribution of the network size in 2000. The mean network size is about 0.7 percent. However, the figure reveals substantial heterogeneity between municipalities with network size ranging from zero to ten percent.

### 1.3.2 Storm Data

I follow Yang (2008) and construct a storm index that measures population-adjusted typhoon exposure at municipality-year level based on meteorological records. In contrast to other measures based on government or news reports, it is not prone to measurement error due to misreporting (Mahajan and Yang, 2019).

I use best-track data provided by the National Oceanic and Atmospheric Administration (NOAA) and the Joint Typhoon Warning Center (JTWC). The data contains the position of every typhoon center in 6h intervals.<sup>15</sup>

I calculate the maximum wind speeds  $w$  for each storm  $s$  in year  $t$ , and grid cell  $i$  in municipality  $m$ , subtract the tropical storm wind speed threshold of 33 knots and normalize it by the maximum wind speed observed in the data ( $w_{max}$ ).

$$x_{i,s,m,t} = \begin{cases} \frac{(w_{i,s,m,t}-33)^2}{(w_{max}-33)^2}, & \text{if } w_{i,s,m,t} \geq 33 \text{ knots} \\ 0, & \text{if } w_{i,s,m,t} < 33 \text{ knots} \end{cases} \quad (1.3)$$

To calculate wind speeds for every typhoon on grid cell level, I apply a physical model by Holland (1980).<sup>16</sup> To obtain population estimates for every grid cell,

<sup>14</sup> I also use other network measures described in more detail in Section 1.3.3 for robustness checks.

<sup>15</sup> Figure A.5 shows a map with best-track data in 6h intervals of typhoons close to the Philippines that is considered for the analysis.

<sup>16</sup> Besides the distance, the ambient and central pressure, the latitude, and the maximum wind speed, also the radius of maximum wind speed enters the model. However, this measure is not provided by NOAA. Therefore, I use JTWC data to train the model for predicting the radius of maximum wind speed, which I then apply to the NOAA data.

data on population is obtained from the Socioeconomic Data and Applications Center (SEDAC). The relevant municipality is determined using a spatial join with the administrative border layer. Because climatologists model the impact of winds on structures typically with a quadratic term (Emanuel, 2005), the numerator and denominator are squared.

I then aggregate the population-weighted grid cell data to municipality-year observations.

$$SI_{m,t} = \frac{\sum_i (pop_{i,m} \sum_s x_{i,s,m,t})}{pop_m} \quad (1.4)$$

Thus, the storm index reflects intensity-weighted events per capita. For example, a storm index of one could correspond to a year in which all residents in a municipality were exposed to a storm with maximum wind speed  $x_{i,s,m,t} = 1$  once or to a year in which all residents of a municipality were exposed to a storm with intensity  $x_{i,s,m,t} = 0.5$  twice. For the ease of interpretation, I use the standardized storm index ( $SSI$ ) for the analysis.

$$SSI_{m,t} = \frac{SI_{m,t} - \mu SI_{m,t}}{\sigma SI_{m,t}} \quad (1.5)$$

Figure A.6 shows that the mean as well as the maximum standardized storm index for the years 1990 to 2015. It reveals that exposure to typhoons increased. This is in line with data from the Emergency Events Database (EM-DAT) provided by the Centre for Research on the Epidemiology of Disasters (CRED).<sup>17</sup> Figure A.7 displays the occurrence of typhoons in the Philippines for the same period and shows a slight upward trend from five typhoons in 1990 to ten typhoons in 2015. Figure A.8 shows a map with the median standardized storm index from 1990 to 2015 for Philippine municipalities as well as incidences of very heavy typhoons. While on average more typhoons occur in the north of the Philippines, there is substantial variation in the timing and location of certain typhoons.

### 1.3.3 Additional Data Sources

*Immigration policy.* To study how legal boundaries affect the role networks can play, I use data from the Immigration Policies in Comparison (IMPIC) dataset (Helbling et al., 2017). The data covers 33 OECD countries between 1980 and 2010. The main advantage of this database is that it not only covers changes in the restrictiveness of

<sup>17</sup> The database combines information from different sources, including UN agencies, non-governmental organizations, insurance companies, research institutes, and press agencies.

immigration regimes but also codes the absolute level.<sup>18</sup> In particular, it is divided into six broad categories (family reunification, labor migration, asylum and refugees, co-ethnics, control, and policy rights). The restrictiveness index is available aggregated as well as for different subcategories and is measured on a numerical scale from zero (unrestricted) to 100 (very restricted). Table A.2 shows summary statistics for the specific policy restrictiveness indices used for the analysis and reveals significant variation in how certain immigration channels are restricted.

*Economic conditions.* I use World Bank data to investigate whether the economic strength of the network is important for the role networks play.<sup>19</sup> This data includes detailed information on different economic measures, compiled from officially-recognized international sources. In particular, I use the data to calculate GDP per capita growth for the destination countries considered in this paper. Figure A.9 shows the distribution of this measure. The mean GDP per capita growth in the period from 2001 to 2015 is about one percent.

*Alternative network measure.* As mentioned before, for the main analysis, I aggregate CFO flow data from 1988 to 2000 to proxy for the network size of a municipality. To get an additional measure of municipality-level network size and to check how sensitive the analysis is with respect to different network measures, I combine different data sets available for the United States.<sup>20</sup> In particular, I use data from several historical passenger lists that include the municipality of birth and the year of immigration. Additionally, I include US Social Security Applications and Claims data that covers information about the municipality of birth for individuals that passed away before 2007. Table A.4 summarizes information about these data. I then aggregate the individual-level data at municipality level to obtain alternative measures of network size for each municipality.<sup>21</sup>

## 1.4 Empirical Analysis

In this section, I describe the empirical setup and introduce the two main specifications for the analysis. The first structural relationship of interest is the effect of typhoon

<sup>18</sup> There are different databases aiming to measure characteristics of immigration policy. The Determinants of International Migration (DEMIG) database covers 45 countries and more than 6,000 policy changes regarding migration for a period from 1945 to 2013. A drawback of this data is that it only provides coding for policy changes and no index for the level of restrictiveness. In contrast, the International Migration Policy And Law Analysis (IMPALA) project covers immigration law and policies from 1960 to 2010 for over 25 countries. However, this data is not yet publicly available.

<sup>19</sup> Data obtained via Stata's `whopendata` command. Sources are the World Bank national accounts data, and OECD National Accounts data files.

<sup>20</sup> Data is obtained from Ancestry.com. Since it is only available for the US, I focus on migration from the Philippines to the US when using this data.

<sup>21</sup> I use the additional network measures in Section 1.4.2 to check whether the results are robust.

exposure on migration. The second specification highlights the key role networks play. I then discuss features of the research design important for the interpretation of the results. Finally, I present the results and discuss the robustness of the findings.

### 1.4.1 Setup

The analysis is based on a panel of 1624 municipalities in the Philippines for the years 2001 to 2015. I estimate a fixed-effects model using exogenous variation in typhoon exposure within municipalities over time. The key identifying assumption is strict exogeneity of typhoon exposure.

*Average effect.* In order to provide causal evidence of the effect of typhoons on migration, I estimate the following equation:

$$y_{m,t} = \alpha + \beta SSI_{m,t-1} + \gamma_m + \eta_t + \epsilon_{m,t} \quad (1.6)$$

where  $y_{m,t}$  is the migration rate in municipality  $m$  in year  $t$  and  $SSI_{m,t-1}$  the standardized storm index in year  $t - 1$ . The standardized storm index serves as a proxy for typhoon exposure. It is lagged by one year because the typhoon season in the Philippines starts in the second half of the year and it may take some time until individuals can realize their intention to migrate. The specification includes municipality fixed effects  $\gamma_m$  to capture any structural differences between municipalities. This is important since Figure A.8 shows that some areas have on average a higher exposure to typhoons than others. To control for overall changes in migration over time, year fixed effects  $\eta_t$  are included.  $\epsilon_{m,t}$  is an i.i.d error term.<sup>22</sup> To investigate heterogeneities between different individuals, I also calculate the migration rate separately for males as well as different age and education groups. Standard errors are clustered at the municipality level.<sup>23</sup>

The sign of  $\beta$  is theoretically ambiguous. On the one hand, typhoons might decrease wealth which makes it more likely that liquidity constraints are binding and migration decreases. On the other hand, typhoons might lower the income of individuals which increases the wage differential between the country of origin and destination and makes it more profitable to migrate. Depending on the relative size of the wealth effect and the income effect,  $\beta$  can thus be positive or negative.

To support the key identifying assumption, Table 1.1 shows the correlation between typhoon exposure in year  $t$  and  $t - 1$ . As already visible in Figure A.8,

<sup>22</sup>In Section 1.4.2, I present robustness checks that include municipality specific linear time trends.

<sup>23</sup>When I discuss the robustness of the findings, I also present results using standard errors clustered at the province level as well as using Conley spatial HAC standard errors to account for spatial and temporal autocorrelation (Conley, 1999).

**Table 1.1:** Correlation between typhoons

	(1)	(2)	(3)	(4)
$SSI_{t-1}$	0.203*** (0.008)	-0.039*** (0.007)	0.289*** (0.007)	-0.000 (0.007)
Observations	24360	24360	24360	24360
Municipality FE	No	Yes	No	Yes
Year FE	No	No	Yes	Yes
# Cluster	1624	1624	1624	1624

Notes: Storm data is based on own calculations using data National Oceanic and Atmospheric Administration (NOAA) and the Joint Typhoon Warning Center (JTWC). The data is aggregated on the municipality level. The table shows coefficients from regressing the std. storm index in  $t$  on the std. storm index in  $t-1$ . Standard errors are clustered at the municipality level.  $*p < 0.1$ ,  $**p < 0.05$ ,  $***p < 0.01$

the positive correlation in column (1) reveals that some areas are more likely to be exposed to typhoons than others. However, once municipality fixed effects are included the correlation becomes less strong. And finally, typhoons in year  $t - 1$  lose all their predictive power for typhoons in year  $t$  once the regression also controls for year fixed effects. The evidence thus suggests that it is as good as random whether a typhoon hits a specific municipality in a certain year.

Figure 1.1 depicts an illustrative example of the identification strategy. Panel (a) displays the average number of migrants 25 months before and after typhoon Haiyan hit the Philippines in November 2013.<sup>24</sup> Panel (b) shows the same data taking out monthly variation. Municipalities are classified into five quantiles depending on how strong they were affected by the typhoon. Figure 1.1 shows that migration from affected and unaffected municipalities evolves very similarly before the typhoon hit the Philippines. However, about half a year later, migration from very strongly affected areas starts to increase significantly. In the peak about 16 months after the typhoon, there were almost 20 migrants per municipality more than if the group would have followed the original trend.

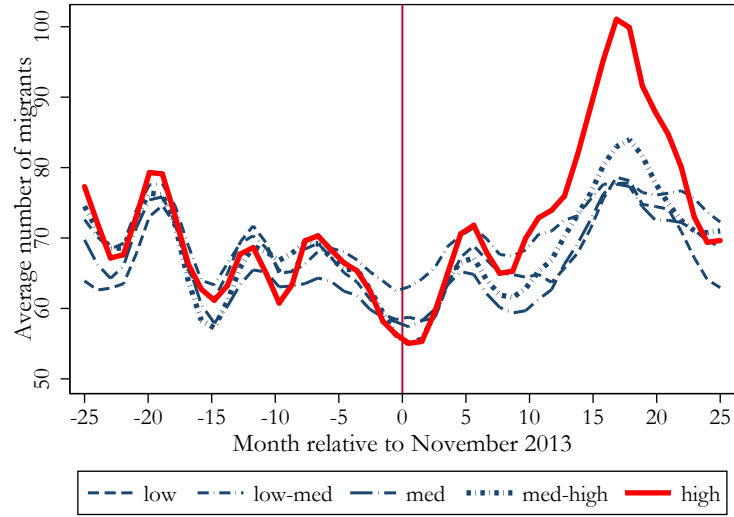
*Network size.* To investigate the role of networks, I exploit cross-sectional variation in the network size of a municipality and estimate the following equation:

$$y_{m,t} = \alpha + \beta SSI_{m,t-1} + \delta (SSI_{m,t-1} \times N_{m,2000}) + \gamma_m + \eta_t + \epsilon_{m,t} \quad (1.7)$$

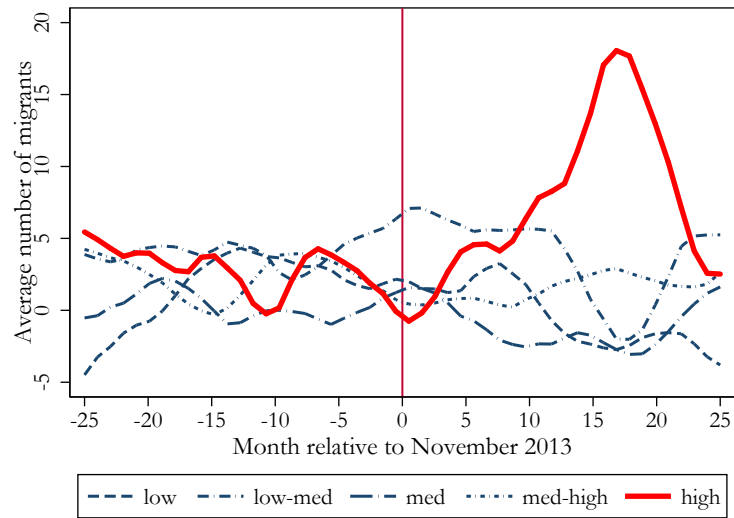
In particular, I interact the standardized storm index with the municipality's network size in the year 2000,  $N_{m,2000}$ . The network measure is composed of the aggregated

<sup>24</sup> Typhoon Haiyan was among the most destructive typhoons that ever made landfall in the Philippines. For more details, see Section 1.2.2.



**Figure 1.1:** Effect of typhoon Haiyan on migration

(a)



(b)

Notes: The analysis is based on raw migration data from CFO for the years 2012 to 2015 aggregated on the month level. Panel (a) shows the average number of migrants of a municipality for differently affected areas using kernel-weighted local polynomial smoothing. Panel (b) shows the residual number of migrants after regressing the number of migrants on month dummies. Typhoon Haiyan hit the Philippines on November 7, 2013. The average exposure to typhoon Haiyan measured by the std. storm index for the different groups were: low: -0.45; low-med: -0.18; med: 0.04; med-high: 0.48; high: 1.57.

number of migrants that left a municipality between 1988 and 2000 divided by the population.<sup>25</sup>

Again, the sign of  $\delta$  is theoretically ambiguous. On the one hand, networks might provide, for example, a legal immigration route to a potential country of destination, which facilitates migration for individuals hit by typhoons. On the other hand, targeted remittances from the network might lower migration intentions.

However, network size might also be correlated with other municipality characteristics that could drive the observed heterogeneity. Therefore, in Section 1.4.2, I also present results of the interaction between the standardized storm index and two other municipality characteristics: population and income. The analysis confirms that the network effect is not driven by these municipality characteristics.

*Municipality versus country analysis.* For the interpretation of the results it is important to keep in mind that the variation comes from typhoon exposure at the municipality level. In contrast, macro studies estimate the average effect of typhoon exposure on migration at the country-level. Consequently, such studies might find no effect of typhoons on migration if typhoons decrease migration in affected areas and increase migration in non-affected areas. The perspective changes if municipality-level variation is considered. Instead of estimating the average effect, I estimate the direct effect of a typhoon on the affected municipality. However, this also means that all municipalities which are not directly exposed to a certain typhoon serve as the comparison group. These municipalities might be indirectly affected by typhoons through trade or the general labor market. Also, psychological reasons such as awareness of a potential threat might play a role.

Thus, the estimated effect is the direct effect net of the indirect effects. If the sign of the effect is the same for directly and indirectly affected areas, the estimated coefficients are lower bounds. However, if the sign is the opposite, the estimated coefficients are upward biased. It could very well be that typhoons decrease migration in directly affected areas and at the same time increase migration in indirectly affected areas, for example, due to the absence of wealth effects in indirectly affected areas. It is, however, rather unlikely that the opposite holds, i.e., that typhoons increase migration in directly affected areas and decrease migration in indirectly affected areas. I interpret the results with these considerations in mind.<sup>26</sup>

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<sup>25</sup>In addition, I show results of interacting the standardized storm index with different network size quantiles.

<sup>26</sup>In Section 1.4.2, I shed light on potential spillovers and present results where I exclude close by but unaffected municipalities from the analysis.

### 1.4.2 Results

#### Effect of Typhoon Exposure on Migration

In this section, I investigate the causal effect of typhoons on migration. To shed light on migrant selection, I also analyze effect heterogeneity with respect to individual characteristics.

Figure 1.2 displays the results. It shows the effect of typhoon exposure on the migration rate for all migrants as well as the effect for different subgroups of the population.<sup>27</sup> The left panel depicts the effect in migrants per 100,000 and the right panel shows the same effect relative to the mean migration rate of the particular group. Instead of group-specific population size, the denominator is always the total population of a certain municipality. Thus, the effects for the different subgroups, e.g., males and females, add to the effect for all migrants.

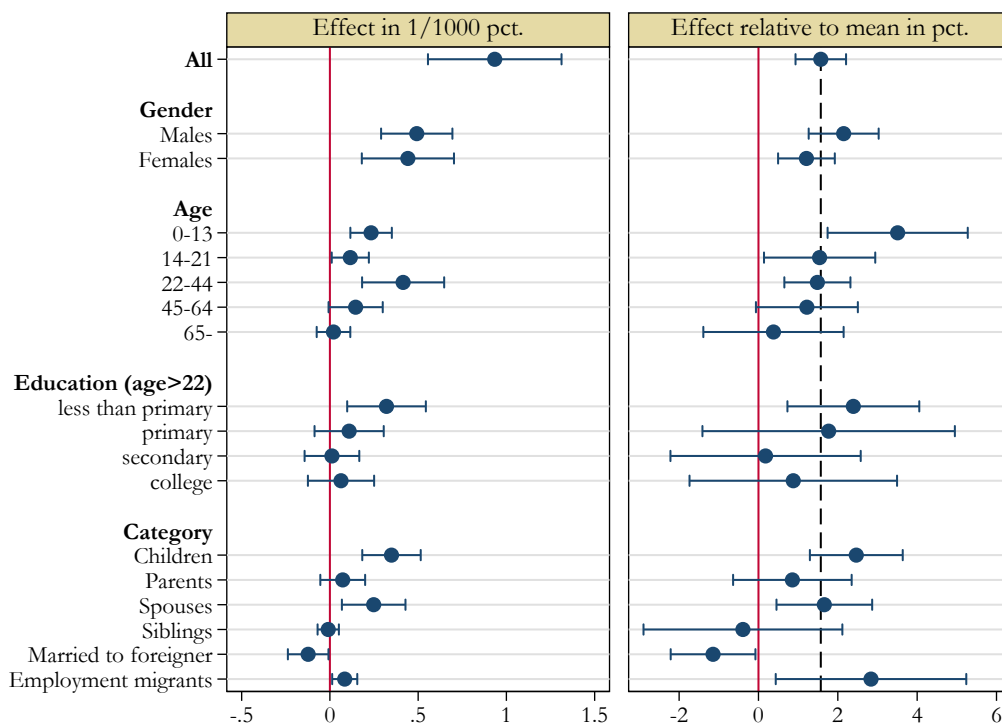
*Average effect.* Figure 1.2 shows that a one standard deviation increase in the standardized storm index leads on average to an increase in the migration rate by about 0.9 individuals per 100,000 or 1.6 percent of the mean respectively. Thus, a negative origin country shock in the form of a typhoon increases migration on average. Mahajan and Yang (2019) find for example that a one standard deviation increase in hurricanes increases worldwide migration to the United States by about 12 percent of the sample mean. Despite the fact that their setting is different and there could be in general heterogeneity in the effect size with respect to origin and destination countries, there is also another explanation that might explain the difference. In contrast to the present study, they use country-level variation in typhoon exposure. If migration also increases in not directly affected areas because of spillovers through trade or general labor market effects, the estimated effect using micro-level variation is likely to be lower.<sup>28</sup>

*Gender.* For both males and females, exposure to typhoons increases migration by about 0.5 individuals per 100,000. However, relative to the mean, the effect for males is larger than for females (2.2 vs. 1.2 percent). One reason for this could be that as a response to typhoons migrants in the country of destination sponsor their spouses and that the gender composition of this sponsoring pattern is different than the usual gender migration pattern. I describe heterogeneities with respect to migrant categories below.

*Age.* In order to investigate whether there is a selection with respect to age,

<sup>27</sup> The corresponding tables are Table A.5, Table A.6, Table A.7, and Table A.8 in the appendix.

<sup>28</sup> In Section 1.4.1, I discuss how spillovers theoretically can affect the estimates and in the robustness checks at the end of this section, I empirically show how the exclusion of close by but unaffected municipalities affects the results.

**Figure 1.2:** Effect of typhoon exposure on migration

Notes: The figure displays the results from estimating Equation 1.6. The dependent variable is the yearly migration rate (in 1/1000 pct.). The independent variable is the standardized storm index in year  $t-1$ . The left panel displays the estimated coefficient  $\beta$ . The right panel shows the estimated coefficient relative to the mean migration rate (in pct.). Coefficients are displayed for all migrants and for different groups of migrants. All regressions include municipality and year fixed effects. Standard errors are clustered at the municipality level. The figure also displays 95 percent confidence intervals.

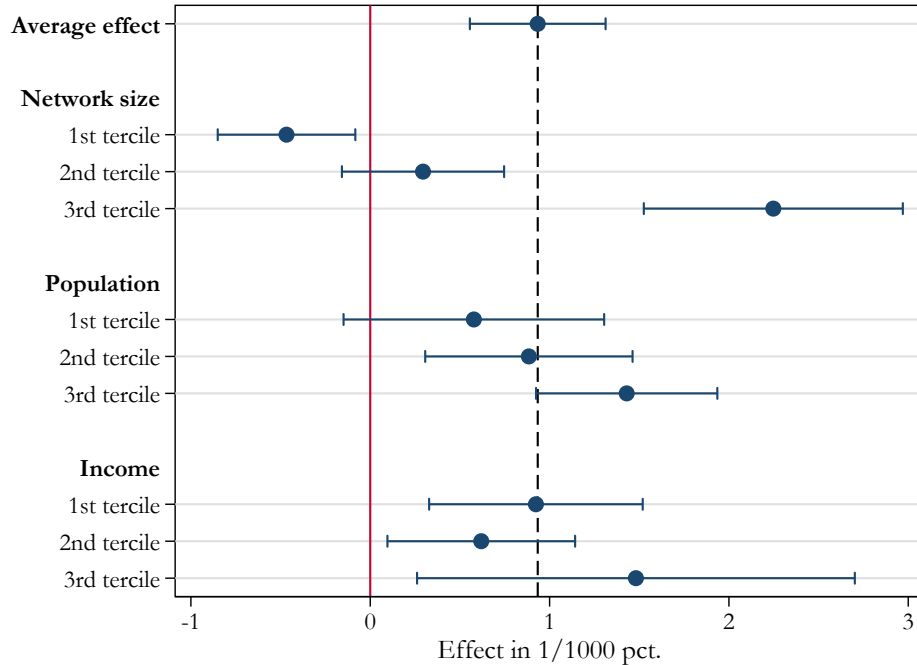
I divide migrants into five age groups (share among total migrants in parentheses): Migrants between 0 and 13 (17%), 14 and 21 (17%), 22 and 44 (44%), 45 and 64 (16%) and migrants 65 or older (6%). Figure 1.2 shows that the effect is positive and significant for all but the oldest age group. In absolute terms, with about 0.4 additional migrants per 100,000, individuals between 22 and 44 are most responsive to typhoons. However, relative to the mean, the migration rate of children between 0 and 13 years increases by 3.5 percent of the mean and therefore reacts strongest to typhoon shocks. This effect could be driven either by parents that migrate and decide to take their children with them or by parents that are already residing in the country of destination and decide to sponsor their children. All other age groups reveal a smaller response of about 1.5 percent of the mean, except individuals 65 or older, who might have a lower willingness to resettle.

*Education.* For the analysis of different education groups, I only consider individuals above the age of 22, who most likely finished their secondary education. The response in migration rates for the education groups varies from 0.2 percent relative to the mean migration rate for individuals with secondary school education to 2.4 percent for individuals with less than primary school education, and only the latter is precisely estimated. Thus, it seems that lower educated individuals react to typhoon shocks more. For a one standard deviation increase in the standardized storm index about 0.3 additional migrants with less than primary school education leave the country.

*Migrant category.* The individual-level migration data also contains information about the relationship between the migrant and her sponsor, i.e., a contact person already residing in the country of destination. For employment-based migrants that person is someone in the sponsoring firm, for family migrants, it is a relative that already lives abroad. Figure 1.2 shows the results for five distinct categories, which in total account for almost 90 percent of permanent Filipino migrants: Spouses (20%), children (34%), parents (10%) and siblings (3%) of former migrants as well as spouses of foreigners (16%) and employment migrants (6%).<sup>29</sup> The effect is positive and significant for spouses, children and employment migrants with effect sizes of 2.5 percent, 1.7 percent and 2.9 percent of the mean. In absolute terms, children of former Filipino migrants are most responsive with almost 0.35 additional children sponsored per 100,000 for every standard deviation increase in typhoon exposure. There is a negative relationship for spouses of foreigners. The effect for parents and siblings is not statistically significant.

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<sup>29</sup> Spouses of foreigners refers in this context to spouses that are sponsored by foreign citizens, not including naturalized Filipinos. The latter are part of the category "Spouses".

**Figure 1.3:** Effect heterogeneity by municipality characteristics

Notes: The figure displays coefficients from regressing the yearly migration rate (in 1/1000 pct.) on the standardized storm index in year  $t-1$  interacted with municipality characteristics. Coefficients are displayed for the average effect and for separate regressions for heterogeneities with respect to network size, population and income level. All regressions include municipality and year fixed effects. Standard errors are clustered at municipality-level. The figure also displays 95 percent confidence intervals.

### The Role of Migration Networks

In the previous section, I shed light on effect heterogeneity between individuals. Now, I investigate the role of established migrant networks in the countries of destination. I estimate Equation 1.7 and interact the storm index with several municipality characteristics. For the ease of interpretation, I classify municipalities into terciles with respect to network size, population, and income and run separate regressions for each heterogeneity. Figure 1.3 displays the coefficients of the interactions between the standardized storm index and the respective dummy variables.<sup>30</sup>

Individuals in municipalities with larger networks show a larger response to typhoon shocks. The migration rate in municipalities in the first tercile slightly decreases and does not significantly change for municipalities within the second tercile. In contrast, for municipalities within the last tercile, a one standard deviation increase in typhoon exposure leads to more than two additional migrants per 100,000. How-

<sup>30</sup> Table A.9 in Appendix A.3 displays the corresponding table and Table A.10 confirms the results using linear interactions.

ever, one might be concerned that other municipality characteristics drive the observed heterogeneity. Obvious candidates are the population size and the income level of municipalities. Figure 1.3 shows that there is little heterogeneity with respect to these characteristics. If anything, individuals in more populated municipalities tend to be more responsive to typhoon exposure.

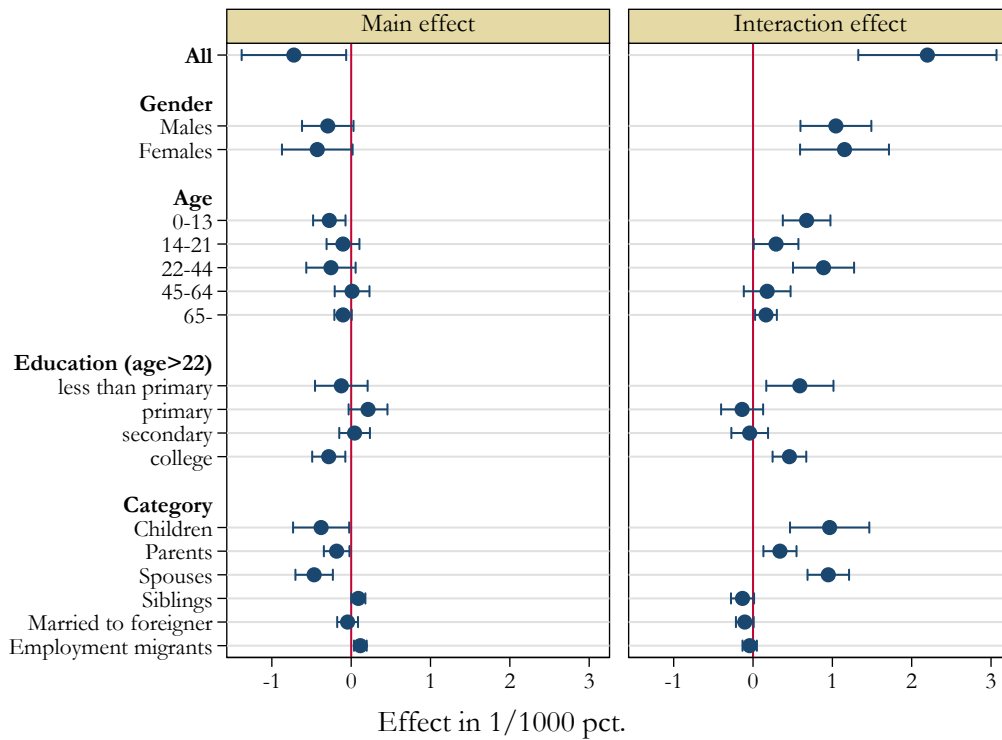
To test whether the heterogeneity with respect to networks remains after controlling for population and income level of a municipality, I turn to a linear specification and include all three municipality characteristics into one regression. Table A.10 summarizes the results. First, it confirms that the average effect is driven by municipalities with larger networks using a linear measure of network size (column (2)). Without any network available, typhoons lead to less migration.<sup>31</sup> Columns (3) and (4) show that neither the interaction term of the standardized storm index with population nor with the income level of a municipality is statistically significant. In column (5), all three municipality characteristics are included. The estimated interaction effect of the standardized storm index and the network size remains economically sizable and statistically significant.

I now investigate whether networks are especially supportive for certain groups of migrants. Figure 1.4 shows the coefficients from estimating Equation 1.7 for all migrants as well as specific groups of migrants. In particular, the left panel depicts the main effect  $\beta$ , and the right panel the interaction effect  $\delta$ .<sup>32</sup> Figure 1.4 shows that the network effect is positive and significant for both males and females. It also highlights the importance of networks for immediate relatives like children, parents and spouses. For these categories, the network effect is particularly strong. The figure also reveals that for persons between 45 and 64, siblings and spouses of foreigners, there is no significant network effect. Interestingly, there is a positive and significant network effect for either very low or very high educated individuals, which is contrary to the finding of McKenzie and Rapoport (2010) who show that especially low skilled Mexican gain from networks when migrating to the US. However, if family reunification is an important channel through which individuals migrate, this might be different because family members already residing in the country of destination might care about the education level of persons they sponsor.

Figure 1.4 also shows that networks do not push employment migration when coping with typhoons. One reason might be that in areas with larger networks, indi-

<sup>31</sup> Figure A.10 indicates that the linear interaction between the standardized storm index and network size is a good approximation for a rather nonparametric approach where network size is divided into 15 quantiles and then interacted with the standardized storm index. While it shows that for half of the municipalities there is a negative or no effect of storms, for the last six quantiles there is a positive and significant relationship.

<sup>32</sup> The corresponding tables are Table A.5, Table A.6, Table A.7, and Table A.8 in the appendix.

**Figure 1.4:** The role of migration networks

Notes: The figure displays the results from estimating Equation 1.7. The dependent variable is the yearly migration rate (in 1/1000 pct.). The independent variables are the standardized storm index in year  $t-1$  and the interaction between the standardized storm index in  $t-1$  with the network size. The figure displays the main effect  $\beta$  on the left and the interaction effect  $\delta$  on the right panel. Coefficients are displayed for all migrants and for different groups of migrants. All regressions include municipality and year fixed effects. Standard errors are clustered at the municipality level. The figure also displays 95 percent confidence intervals.



viduals might not need to migrate through the employment-based migration channel but can rather rely on family reunification channels.

### Robustness

*Weighting and time trends.* One might be concerned that different municipality-specific migration trends could bias the results. However, columns (2) to (4) in Table A.11 show that the results are robust to the inclusion of municipality-specific linear time trends. While the average effect decreases when time trends are included, it remains statistically significant at the 5 percent level. On the other hand, if I use the population size in the year 2000 as a weight, the average effect increases (see columns (3) and (4)). In contrast, the network effect remains remarkably stable to both changes.

*Internal Migration.* For the main analysis, I calculate migration rates based on individuals birthplace. However, individuals might internally migrate during their lifetime for any reason before migrating to another country. Because I attribute those internal movers to the wrong municipality, there is attenuation bias. Given the data available, I can also aggregate the data using the current municipality (at the time of registration at CFO). Although it is likely that the bias is even larger in the latter case, I present results using this alternative aggregation in column (5) of Table A.11. The results are robust to the alternative measurement, and if anything, the network effect becomes larger.

*Standard errors.* One might be concerned that clustering at the municipality level is insufficient. I address this issue in two ways: First, I cluster the standard errors at the province level to allow for the error terms to be correlated on a greater regional level. Column (6) Table A.11 shows that the results are robust. Second, to account for spatial and serial correlation in the error term, I re-estimate the main specifications using Conley spatial HAC standard errors (Conley, 1999). Spatial autocorrelation is assumed to decrease linearly up to a 100 kilometers cut-off. Serial correlation is assumed to decrease linearly for up to 15 years. Column (7) of Table A.11 shows that the results hold.

*Spillovers.* A concern might be that some municipalities not directly affected by a typhoon might be affected indirectly, for example, because of effects on the local economy. To investigate how this affects the results, I exclude municipalities close to municipalities that are hit by a heavy typhoon. In particular, I classify typhoons above the 75th quantile as heavy.<sup>33</sup> Table A.13 shows the effects for different exclusion radii

<sup>33</sup>In Table A.12, I investigate the effect of heavy typhoons. The results show that heavy storms increase migration by three persons per 100,000 or five percent of the mean migration rate. Again, this effect is entirely driven by individuals from municipalities with larger networks. In above-median network-size municipalities, the effect increases to about five persons per 100,000.

(from 20 up to 100 km). The larger the radius for excluding close by municipalities, the larger the effect of typhoons on migration. The estimated effect becomes almost twice as large compared to the baseline specification if not directly affected municipalities in a radius of 100 km are excluded. It thus seems that also individuals in municipalities indirectly affected migrate more in response to typhoons.

*Alternative network measures.* In the main analysis, network size is determined by CFO flow data from 1988 to 2000. As a robustness check, I use data from the United States Social Security Applications and Claims Index. The data consists of people who died in the US and contains information about the date and place of birth at the municipality level. Unfortunately, there is no information about the immigration year. To measure the network size of a municipality in the year 2000, I take all individuals who died in the US before 2001 and aggregate the number of individuals at the municipality level. The underlying assumption for this approach is that people who died are a good proxy for the population of immigrants. However, this may overestimate long-established networks since older persons are more likely to die. Therefore, this complements the analysis using the flow data from 1988 to 2000, which captures more recent networks. Additionally, I use data on migration flows from the early 1880s until the 1960s from US passenger lists, which capture migrant networks established a very long time ago.<sup>34</sup> Table A.15 shows that for the US only, the network effect is positive and significant. Reassuringly, the findings using the Social Security data are very similar in magnitude compared to the main results. In contrast, the network measure calculated based on the passenger lists does not seem to be an important driver of the effect.

## 1.5 Mechanisms

The previous results suggest that network size is important. To shed light on the mechanisms through which networks influence migration, I turn to a destination-specific analysis.<sup>35</sup> First, I outline the specification. Then, I focus on two key mechanisms brought forward in the literature: Providing a legal route to immigration and providing economic support.

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<sup>34</sup> Table A.14 in the appendix shows that the correlation between the different network measures varies significantly. While the pairwise correlation coefficient between the social security network measure and the passenger list is 0.76, the correlation with the main network measure is 0.54. The correlation between the main network measure and the passenger list is only 0.28.

<sup>35</sup> Table A.16 replicates the main results for the sample with country of destination specific networks. Reassuringly, the relationships established before with respect to typhoon exposure and network size also hold for the destination country-specific analysis.

### 1.5.1 Setup

In the destination country analysis, one observation is a municipality-year-destination country combination.

$$y_{m,d,t} = \alpha + \beta \text{SSI}_{m,t-1} + \delta (\text{SSI}_{m,t-1} \times N_{m,d,2000}) + \theta (\text{SSI}_{m,t-1} \times N_{m,d,2000} \times F_{d,t}) + X_{m,d,t} + \epsilon_{m,d,t} \quad (1.8)$$

The migration rate  $y_{m,d,t}$  is measured as the number of individuals per 100,000 who migrate from a certain municipality  $m$  to a specific destination country  $d$  in year  $t$ . Network size  $N_{m,d,2000}$  is calculated at the municipality-country level and is a proxy for the migrant stock in a specific destination country.  $\text{SSI}_{m,t-1}$  is the standardized storm index in year  $t - 1$ . The effect of interest is the triple interaction of the standardized storm index, the destination country-specific network size, and the exogenous factor  $F_{d,t}$ . The regression includes municipality-destination country as well as year fixed effects.<sup>36</sup>

The exogenous factors of interest are the destination country's immigration regime measured by the policy restrictiveness index  $\text{PRI}_{d,t-1}$  and the networks economic strength proxied by the average GDP growth  $\text{GDPG}_{d,t-5-t-1}$  ( $\text{GDPG}_{d,t-10-t-1}$ ) in the previous 5 (10) years. Since the policy in place is relevant at the time of application, not actual migration, I lag the policy restrictiveness index by one year.<sup>37</sup> The coefficient for  $F_{d,t}$  and the double interaction terms of  $\text{SSI}_{m,t-1}$ ,  $N_{m,d,2000}$  and  $F_{d,t}$  are included in  $X_{m,d,t}$ . The hypothesis is, that the migration facilitating role of networks increases in the openness of immigration regimes regarding the family channel and in the economic strength of networks.

### 1.5.2 Providing Legal Entry

Mahajan and Yang (2019) show that one key mechanism through which networks operate is by providing legal entrance to the US. While they look at different entry

<sup>36</sup> Bertoli and Fernández-Huertas Moraga (2013) raise the issue that migration flows between pairs of countries do not only depend on the attractiveness of a specific destination country but rather on the relative attractiveness of the destination countries. Following the terminology of Anderson and van Wincoop (2003), who discussed this in the context of international trade, Bertoli and Fernández-Huertas Moraga (2013) call this phenomenon multilateral resistance to migration. According to them, multilateral resistance can cause a bias in the estimates of determinants of bilateral migration flows. However, as Beine et al. (2015) point out, including country of origin fixed effects account for the effect of multilateral resistance, if it does not vary across destination countries (see Bertoli and Fernández-Huertas Moraga (2013) for a discussion). Furthermore, Beine (2016) as well as Bertoli and Fernández-Huertas Moraga (2015) show that multilateral resistance is a minor concern when estimating network effects.

<sup>37</sup> Because data is only available until 2010, I take the 2010 restrictiveness index for the years 2010 to 2015.

**Table 1.2:** The role of policy restrictiveness

	Family			Labor		
	Family members	Quotas	Financial requirem.	Tests	Job offer	Funds
$SSI_{t-1}$	-0.04*** (0.01)	-0.07*** (0.01)	-0.14*** (0.01)	0.01 (0.02)	-0.00 (0.02)	-0.07*** (0.01)
$SSI_{t-1} \times \text{Network}_{2000}$	3.96*** (0.69)	7.18*** (0.77)	11.59*** (1.50)	-4.36*** (0.90)	-12.72*** (1.87)	-1.04** (0.52)
$PRI_{t-1}$	0.02*** (0.00)	-0.02*** (0.00)	-0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.00*** (0.00)
$SSI_{t-1} \times PRI_{t-1}$	-0.00*** (0.00)	0.00 (0.00)	0.00*** (0.00)	-0.00*** (0.00)	-0.00** (0.00)	0.00*** (0.00)
$\text{Network}_{2000} \times PRI_{t-1}$	-9.91*** (0.70)	0.22 (0.34)	0.21 (0.95)	1.43*** (0.22)	0.32 (0.24)	-1.40*** (0.33)
$SSI_{t-1} \times \text{Network}_{2000} \times PRI_{t-1}$	-0.11*** (0.03)	-0.11*** (0.02)	-0.14*** (0.02)	0.24*** (0.03)	0.27*** (0.03)	0.33*** (0.04)
Observations	462840	462840	462840	462840	462840	414120
# Cluster	1624	1624	1624	1624	1624	1624
Mean dependent variable	2.99	2.99	2.99	2.99	2.99	2.99

Notes: The analysis is based on individual migration data from CFO for the years 2001 to 2015 aggregated at municipality and destination country level. Population data is from the Philippine Censuses and interpolated for the years without census. Data for the Policy Restrictiveness Index is from the Immigration Policies in Comparison (IMPIC) database. The policy restrictiveness indices (PRI) are measured in percent with zero being not restrictive and 100 being very restrictive. The dependend variable is destination country specific yearly migration rate (in 1/1000 pct.). The regression includes municipality-destination country and year fixed effects. Standard errors are clustered at municipality-level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

classes through which immigrants come, I investigate this mechanism by looking at the restrictiveness of immigration regimes for different destination countries.

On the one hand, the restrictiveness of immigration regimes limits how networks can operate. On the other hand, if immigration policies are very restrictive, individuals might especially rely on networks. Therefore, it is useful to consider different aspects of the immigration system. The IMPIC database provides indices of restrictiveness for different immigration categories. For the analysis, I consider family and labor migration policies. Within these broad categories, I look at specific areas. In the family category I investigate policies regarding family members, quotas, and financial requirements, and in the labor category I analyze policies regarding labor market tests, job offers and financial funds.<sup>38</sup> The policy restrictiveness index (PRI) has a numerical scale from zero (unrestricted) to 100 (very restricted).

I estimate Equation 1.8 for the different restrictiveness indices. Table 1.2 shows that the more open a destination country is in terms of family reunification, the more

<sup>38</sup> Table A.3 describes the categories that are used for the analysis and Table A.2 displays the summary statistics for these respective indices.

important is the network effect when a municipality is hit by a typhoon. In particular, it seems that networks facilitate more migration in response to typhoons if more kinds of family members are allowed to be sponsored (column (1)). A decrease from the 75th quantile to the median restrictiveness of this measure, i.e., a decrease in eight points, increases the interaction effect of typhoon exposure and network by almost 25 percent. There is also an amplifying of the network effect if less restricted family reunification policies are in place (column (2)) or if there are less strict financial requirements for family migrants (column (3)).

However, the opposite applies when looking at restrictions regarding labor migration. The evidence suggests that the more restrictive immigration policies for employment-based migrants are, the more helpful are networks when hit by a typhoon. In particular, networks seem to be supportive if job applicants are tested (column (4)), if job offers are required to be considered for immigrating via an employment-based visa (column (5)), and if financial funds are required (column (6)). Thus, it seems to be the case that there is a substitution between relying on networks and migrating via employment-based channels.<sup>39</sup>

### 1.5.3 Providing Economic Support

Sometimes networks have more supporting power than at other times. For example, it might be easier for a network to help cover migration costs or to find a job if individuals in the network are economically successful. Since I do not observe the success of individuals in the destination country, I take past GDP per capita growth in the country of destination as exogenous variation in the economic success of individuals in the destination country. It thus serves as proxy for the economic success of the network. In particular, I measure past GDP per capita growth for every country-specific municipality network.<sup>40</sup>

Table A.19 shows for the case of Filipino immigrants to the US, that past GDP per capita growth has a significant effect on wage income and employment. If average GDP per capita growth in a certain state in the last ten years is one percentage point higher, employment increases by 0.4 percentage points and wage income by 1.4 percent.

Table 1.3 includes the triple interaction of standardized storm index, network size, and GDP per capita growth as well as all pairwise interactions. I find that the interaction effect of the standardized storm index and the network size is stronger if

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<sup>39</sup>Table A.17 and Table A.18 in the appendix show that the results are robust to using the average policy restrictiveness index from 2001 to 2010 or the policy restrictiveness index from 2000.

<sup>40</sup>It might well be that economically more successful networks also send more remittances which might lower migration intension. However, I do not observe remittances and I am thus not able to disentangle these effects. The estimated effect thus reflects the sum of these two factors.

past GDP per capita growth is higher. This holds for the case of more recent average GDP per capita growth in years  $t-5$  to  $t-1$  in column (1) and more long term average GDP per capita growth in years  $t-10$  to  $t-1$  in column (2). In both cases networks do not seem to amplify migration in response to typhoons in the case of negative or close to zero GDP per capita growth. However, for every percentage point increase in GDP per capita growth the interaction effect of typhoon exposure and network size increases by about one. It thus seems that it is easier for economically more successful networks to help individuals exposed to typhoons to migrate than for less successful networks.

**Table 1.3:** The role of economic success

	Average GDP growth t-5 to t-1	Average GDP growth t-10 to t-1
$SSI_{t-1}$	0.02* (0.01)	0.06*** (0.01)
$SSI_{t-1} \times \text{Network}_{2000}$	0.66 (0.42)	0.18 (0.70)
GDP growth	-0.03*** (0.01)	-0.08*** (0.00)
$SSI_{t-1} \times \text{GDP growth}$	-0.05*** (0.01)	-0.05*** (0.01)
$\text{Network}_{2000} \times \text{GDP growth}$	-1.02* (0.61)	0.99 (0.72)
$SSI_{t-1} \times \text{Network}_{2000} \times \text{GDP growth}$	1.09*** (0.35)	1.24*** (0.43)
Observations	462840	462840
# Cluster	1624	1624
Mean dependent variable	2.99	2.99

Notes: The analysis is based on individual migration data from CFO for the years 2001 to 2015 aggregated at municipality and destination country level. Population data is from the Philippine Censuses and interpolated for the years without census. GDP data is from the World Bank national accounts data, and OECD National Accounts data files. The dependend variable is destination country specific yearly migration rate (in 1/1000 pct.). Average GDP growth is measured in percent. The regression includes municipality-destination country and year fixed effects. Standard errors are clustered at municipality-level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

## 1.6 Conclusion

Typhoons push millions of people into poverty each year and the destructiveness of typhoons will increase as climate change progresses. In this paper, I study the impact of typhoons on migration in the Philippines. I combine unique individual-level migration data with a sophisticated measure of typhoon exposure based on meteorological records. I exploit the richness of the data to shed light on the direct effect and abstract from more indirect macro-level effects.

I find that with every standard deviation increase in typhoon exposure, migration increases by about one per 100,000 individuals. On the one hand, migrants leave the country using employment-based channels. On the other hand, family members already in the country of destination sponsor their children, parents and spouses, which highlights the key role of migrant networks.

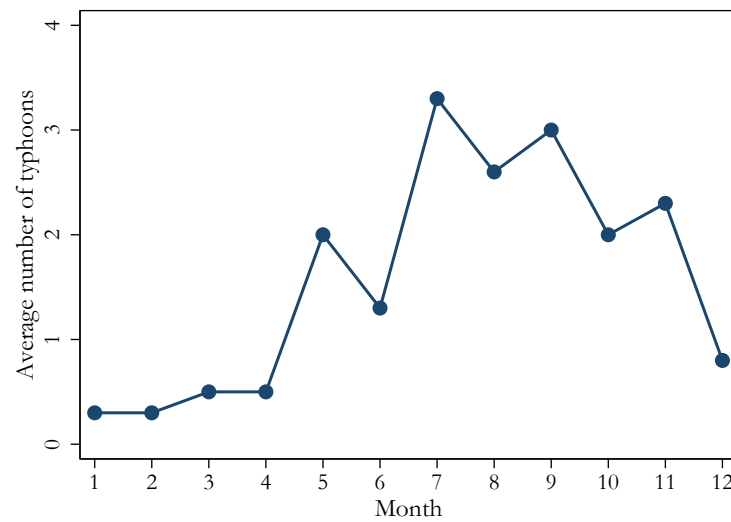
However, networks are not always able to fully facilitate migration. To shed light on the mechanism through which networks operate, I combine exogenous shocks in the country of origin with exogenous destination country factors. I find that if immigration policies concerning family reunification are more restrictive, the supportive power of the network decreases which is consistent with the idea that providing legal entrance to a potential destination country is a key mechanism. Moreover, the network effect increases if the network is economically successful.

It might thus well be that at different times, equally sized networks provide different support. Therefore, it is crucial for studying migration responses to any shocks, to consider the situation in potential destination countries. Understanding the role of networks is key for policy makers, especially in the country of destination. If regions with a large diaspora in certain destination countries are hit by natural disasters, migration flows are likely to concentrate to these destinations. The findings of this paper thus help predict future migration flows.

## Appendix A

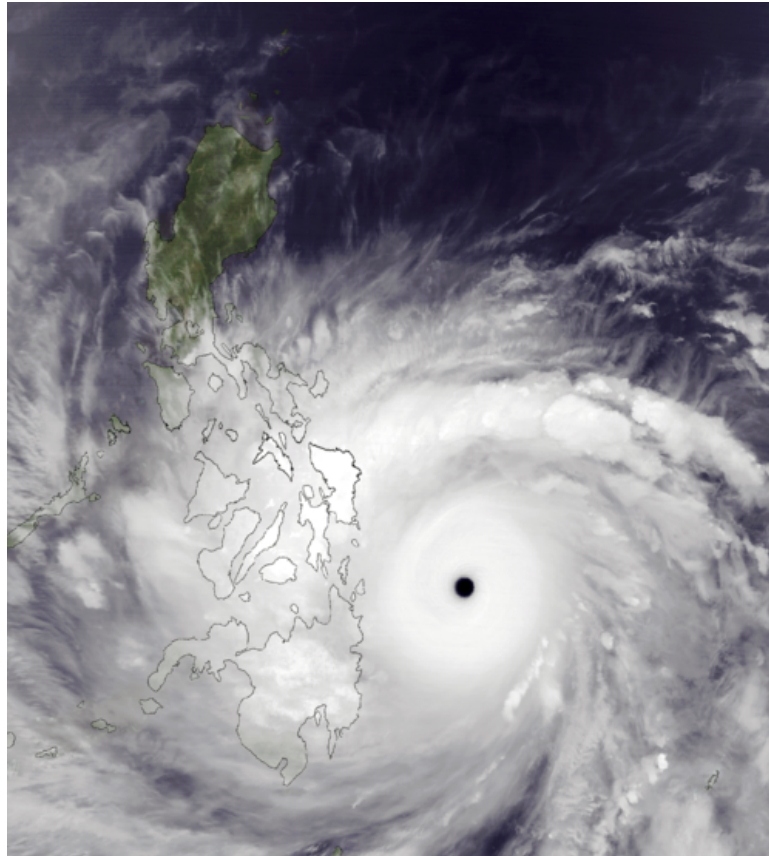
### A.1 Background

**Figure A.1:** Typhoon activity in the Philippines

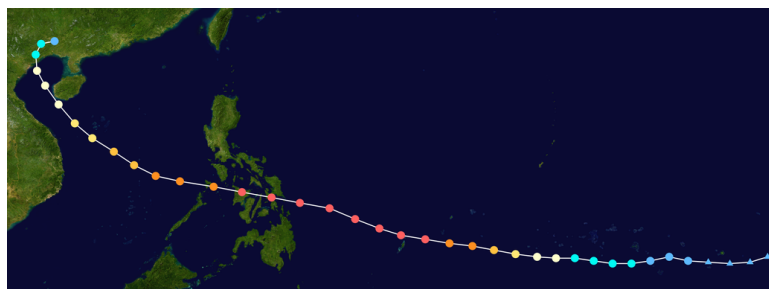


Source: Philippine Atmospheric, Geophysical and Astronomical Services Administration, Period 2000-2009.



**Figure A.2:** Typhoon Haiyan striking the Philippines

(a)

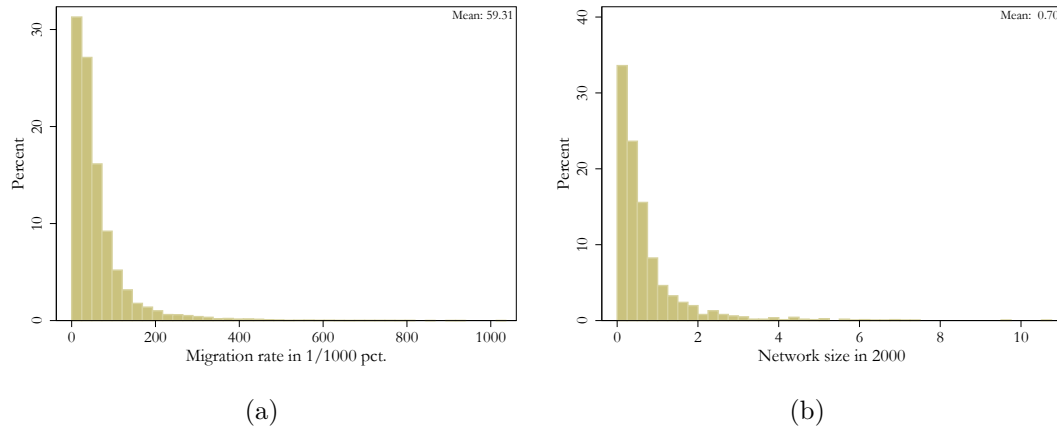


(b)

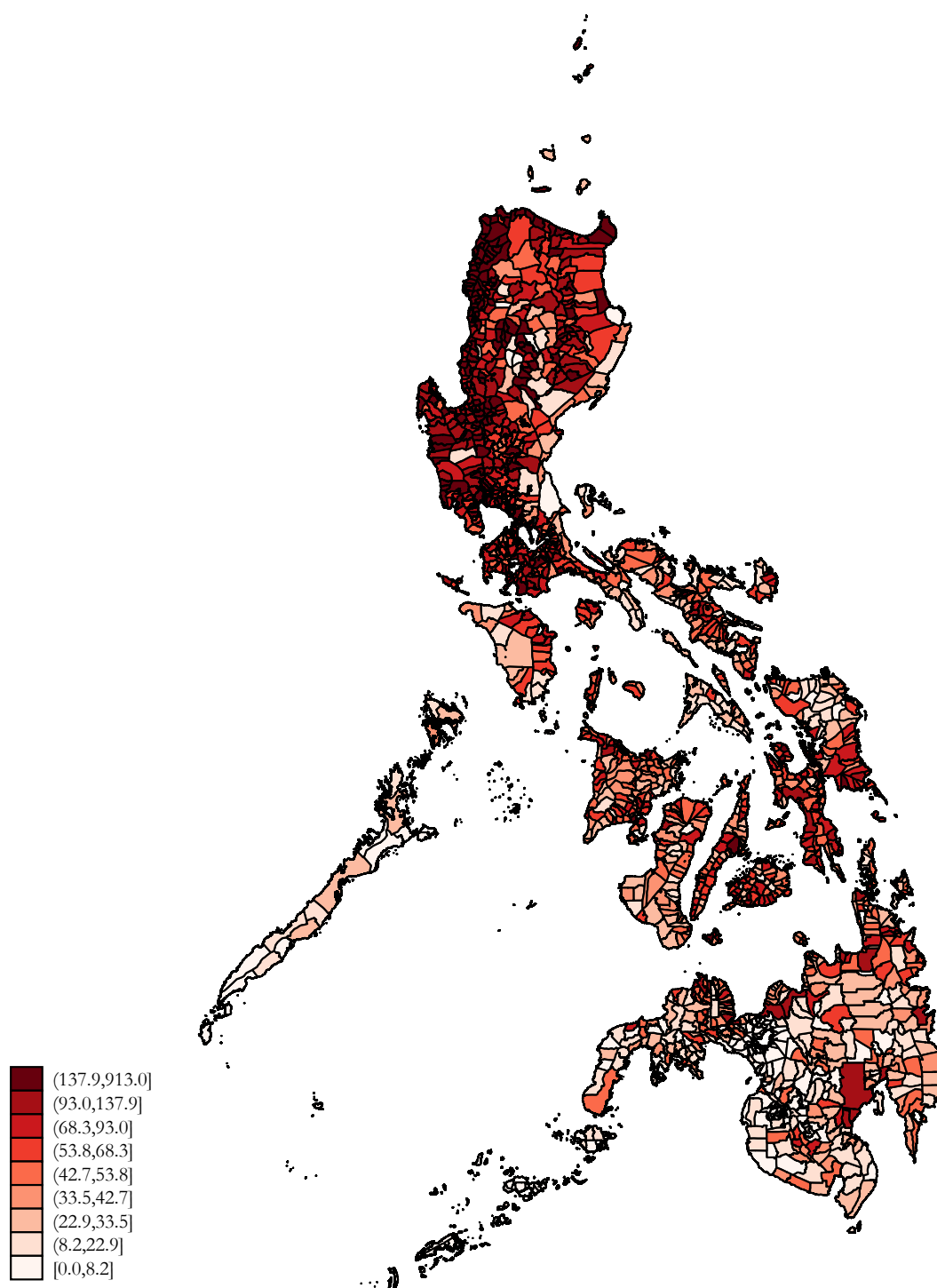
Notes: Panel (a) shows Typhoon Haiyan at peak intensity and approaching the Philippines on November 7, 2013. Source upper photo: Nasa. Panel (b) shows a track map of Typhoon Haiyan with the location of the storm at 6-hour intervals. The color corresponds to the typhoon's maximum wind speeds as classified in the Saffir–Simpson scale. It ranges from light blue (tropical depression, i.e.  $<62$  km/h) to red (category 5, i.e.  $>252$  km/h). Source lower photo: WikiProject Tropical cyclones/Tracks, with the background image being from NASA and tracking data from JTWC.

## A.2 Data

**Figure A.3:** Distribution of migration rate and network size



Notes: The data is based on individual migration data from CFO for the years 1988 to 2015 aggregated at municipality-level. Panel (a) shows a histogram of the migration rate of municipalities from 2001 to 2015 (in 1/1000 pct.). Panel (b) shows a histogram of the measure of the network size of municipalities. It is the cumulative number of migrants from a municipality between 1988 and 2000 in percent.

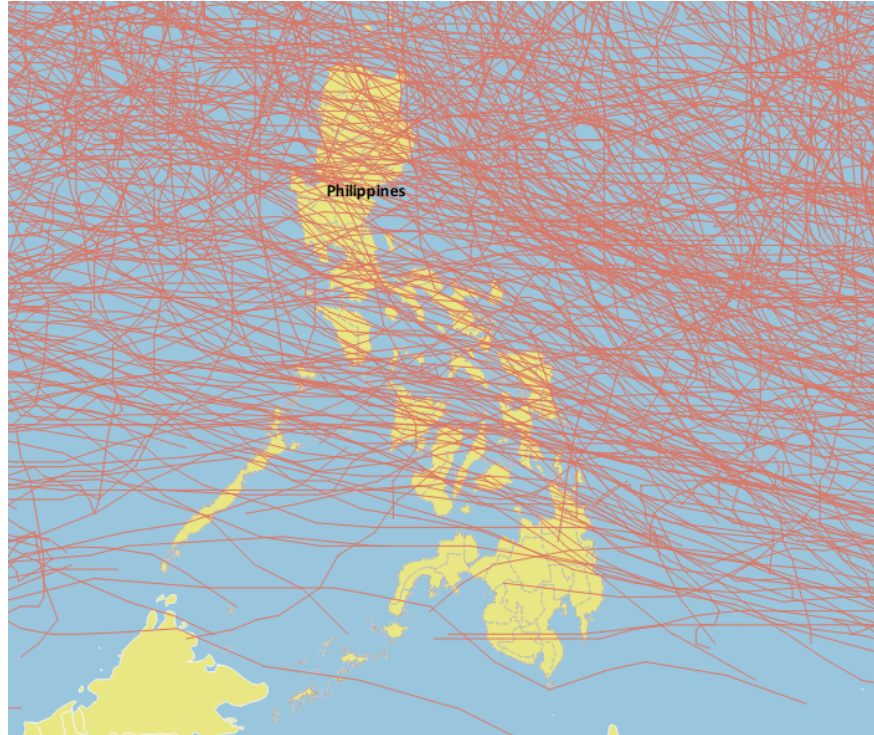
**Figure A.4:** Map of migration rate by municipality, 2015

Notes: The analysis is based on individual migration data from CFO for the year 2015 aggregated at municipality-level. The map shows the migration rate of municipalities (in 1/1000 pct.).

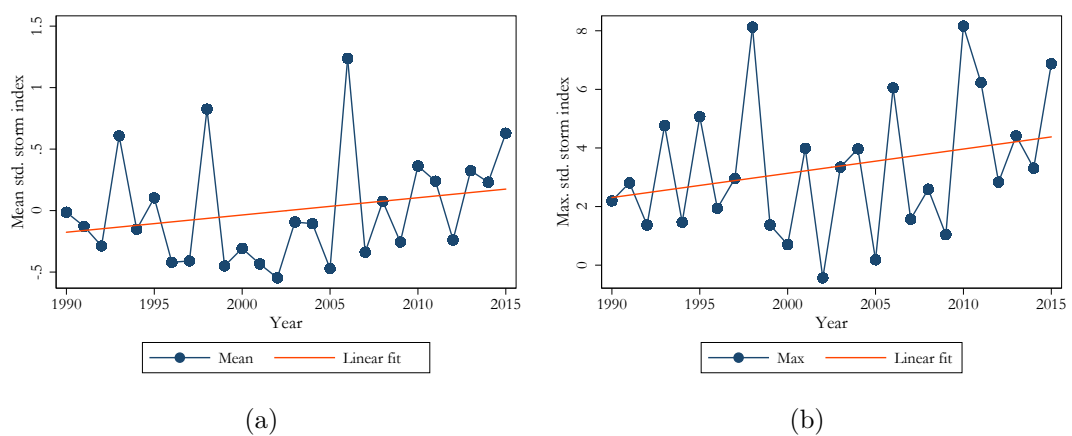
**Table A.1:** Summary statistics

	<b>1988-2015</b>		<b>2001-2015</b>	
	Mean	SD	Mean	SD
<b>Migration data</b>				
Migration rate in 1/1000 pct.	60.1	(78.5)	59.3	(71.3)
Number of migrants p.a.	39.9	(235.9)	44.8	(233.8)
Number of marriage migrants p.a.	4.6	(24.2)	7.1	(30.3)
Number of children p.a.	13.7	(102.2)	15.4	(97.0)
Number of parents p.a.	3.7	(13.5)	4.3	(14.4)
Number of spouses p.a.	8.0	(38.5)	9.2	(37.6)
Number of siblings p.a.	2.2	(11.5)	1.5	(7.0)
Average age	35.5	(8.6)	35.6	(8.2)
Share males	0.3	(0.2)	0.3	(0.2)
Share at least university completed	0.8	(0.2)	0.8	(0.2)
Share at least secondary education (25-65)	0.8	(0.2)	0.8	(0.2)
Share at least secondary education (25-45)	0.9	(0.2)	0.9	(0.2)
<b>Census data</b>				
Population in thous.	51.2	(108.4)	59.3	(124.5)
Share males	0.5	(0.01)	0.5	(0.01)
Average age	24.6	(2.2)	26.4	(2.1)
Literacy rate	0.9	(0.10)	0.9	(0.07)
Share of internet availability	0.04	(0.06)	0.04	(0.06)
Share of tv availability	0.4	(0.3)	0.6	(0.2)
Average years of schooling	6.3	(1.2)	7.1	(1.1)
Share oversea worker	0.01	(0.01)	0.02	(0.01)
Share at least secondary education	0.4	(0.2)	0.5	(0.2)
Share at least secondary education (25-65)	0.4	(0.2)	0.5	(0.2)
Share at least secondary education (25-45)	0.4	(0.2)	0.5	(0.2)
Average family size	6.0	(0.5)	5.6	(0.4)
Share employed (in labforce)	0.6	(0.03)	0.6	(0.03)
Share married	0.4	(0.03)	0.4	(0.03)
<b>Storm data</b>				
Std. storm index	-0.0007	(1.0)	0.04	(1.1)
Std. neighbouring storm index	-0.005	(1.0)	0.05	(1.1)
Observations	45752		24510	

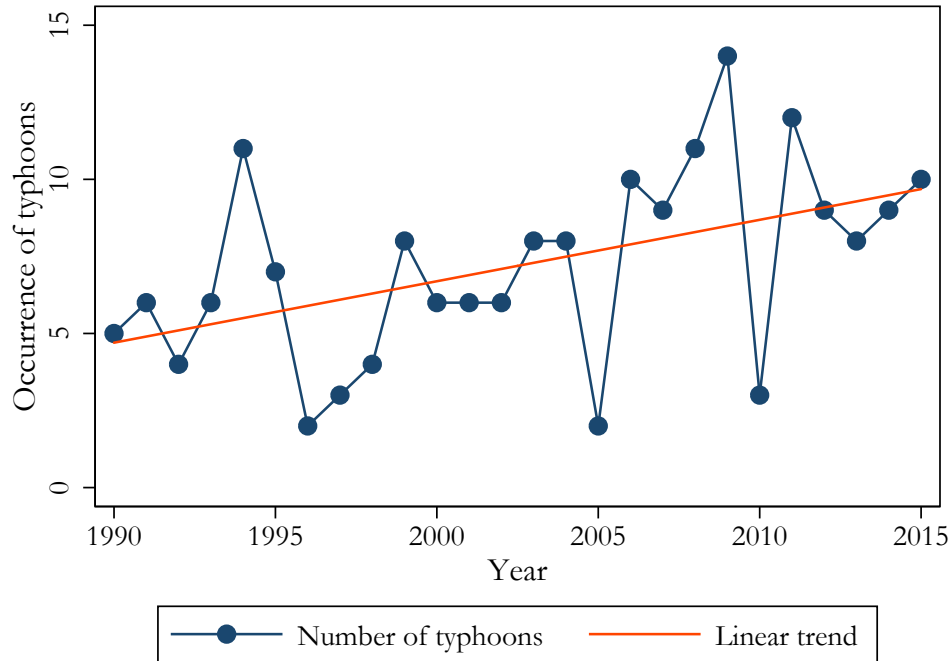
Notes: The migration data is based on individual migration data from CFO for the years 1988 to 2015. Census data for the years 1990, 1995, 2000, 2010 and 2015 comes from the Philippine Census. Storm data is based on own calculations using data National Oceanic and Atmospheric Administration (NOAA) and the Joint Typhoon Warning Center (JTWC). All variables are aggregated at the municipality level.

**Figure A.5:** Best-track typhoon data

Notes: The map shows the route of all typhoons between 1990 and 2015 using best-track data in 6h intervals from the National Oceanic and Atmospheric Administration (NOAA).

**Figure A.6:** Standardized storm index 1990-2015

Notes: Storm data is based on own calculations using data National Oceanic and Atmospheric Administration (NOAA) and the Joint Typhoon Warning Center (JTWC). The left panel shows the mean standardized storm index for the years 1990 to 2015 and the right panel the max standardized storm index.

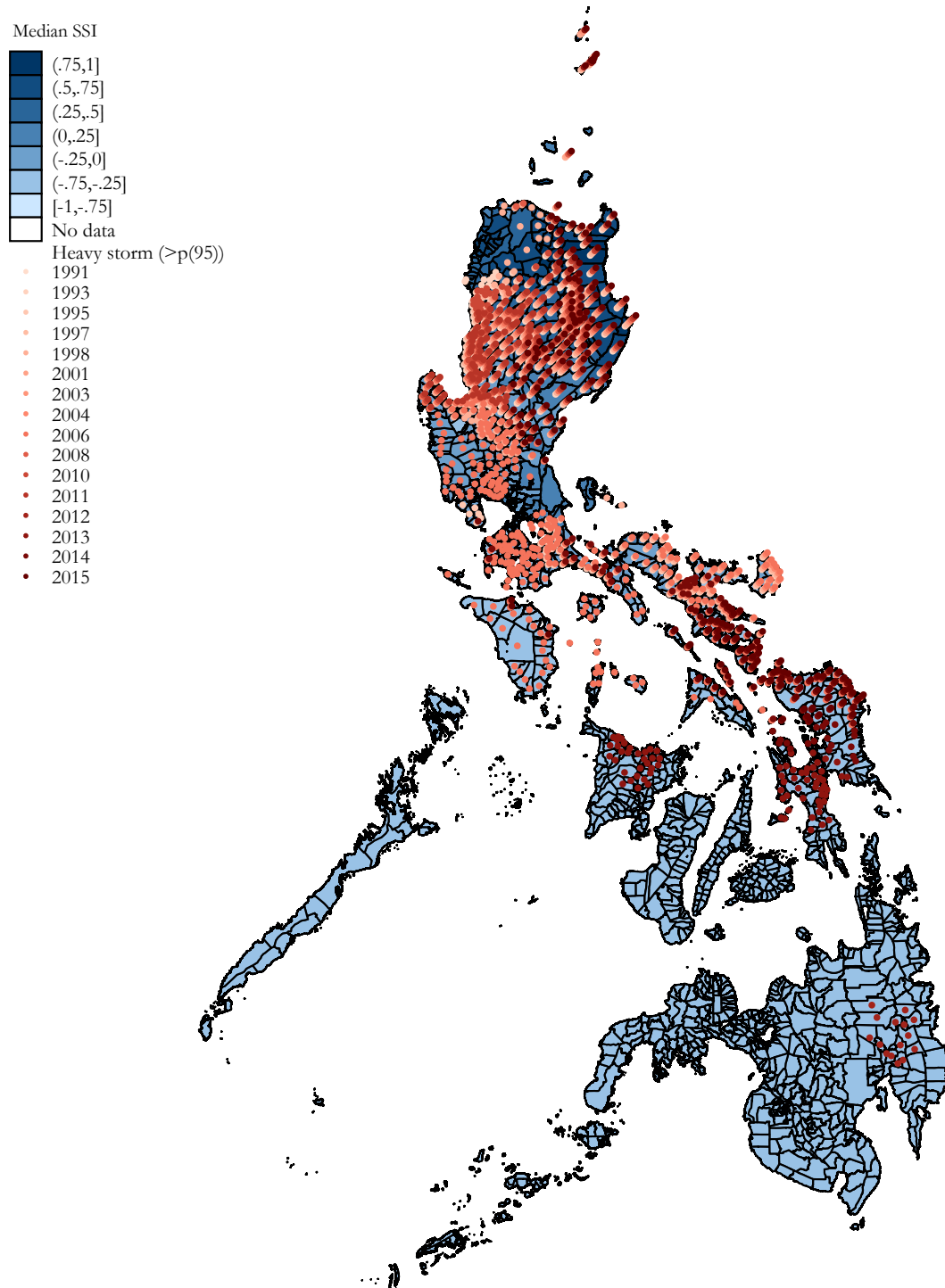
**Figure A.7:** Occurrences of typhoons in the Philippines, EM-DAT data

Source: Emergency Events Database (EM-DAT) provided by the Centre for Research on the Epidemiology of Disasters (CRED).

**Table A.2:** Summary statistics for selected policy restrictiveness indices

	Mean	Sd	Min	Max	Count
<b>Family</b>					
Family member	23.5	31.1	0	100	589
Quotas	19.3	34.8	0	100	588
Financial requirements	41.0	33.8	0	100	589
<b>Labor</b>					
Tests	38.7	28.7	0	100	589
Job offer	64.6	22.6	0	100	584
Funds	16.7	26.9	0	100	527

Notes: The table shows summary statistics for the Policy Restrictiveness Index for different immigration categories from the Immigration Policies in Comparison (IMPIC) database. The data consists of the 19 destination countries relevant for the analysis for the years 2000 to 2010. The indices measure the restrictiveness of immigration policies with 0 being very open and 100 being very restrictive.

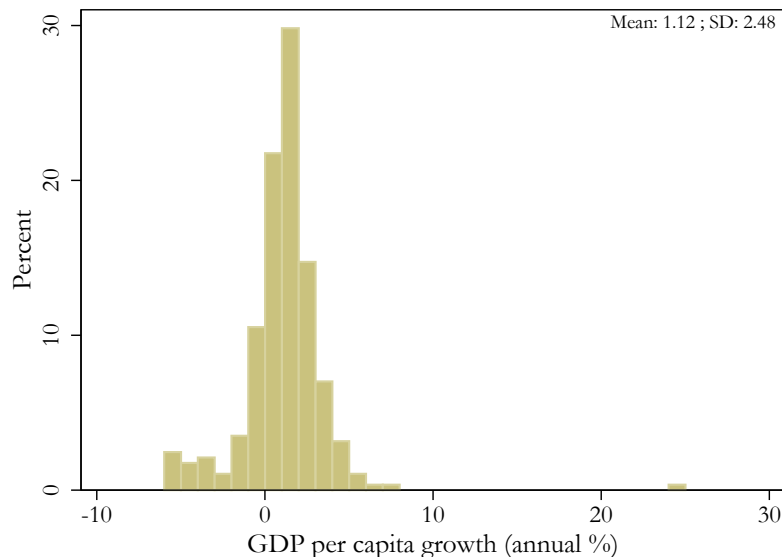
**Figure A.8:** Map of typhoon exposure by municipality, 1990-2015

Notes: Storm data is based on own calculations using data National Oceanic and Atmospheric Administration (NOAA) and the Joint Typhoon Warning Center (JTWC). The map shows the median standardized storm index from 1990 to 2015 as well as occurrences of heavy storms, i.e. storms above the 75th percentile of the storm distribution.

**Table A.3:** Description of categories in the IMPIC database

Category	Subcategory	Description
Family	Family members	Which family members were allowed to immigrate according to the regulations governing family reunification? Please also consider family members who are allowed to immigrate under certain conditions only.
Family	Quotas family reunification	Were there quotas (numerical limits) on the overall number of sponsored persons?
Family	Financial requirements	Were sponsors required to prove the ability to financially support themselves and their family? If yes, please specify how.
Labor	Labor market tests	Did your country use a labor market test (i.e. job applications are tested against the available pool of eligible workers for the job opening to make sure no settled worker could do the job)?
Labor	Job offer	Was a concrete job offer (e.g. acceptance letter, formal invitation) or a contract signed in advance required or beneficial for immigrating?
Labor	Specific financial funds	Did migrant workers need to prove the ability to support themselves? Such a proof might concern the fact that a specific income per month or a certain amount of financial funds is required.

Source: The Immigration Policies in Comparison Dataset Technical Report (Bjerre et al., 2016).

**Figure A.9:** Average GDP per capita growth of networks, 2001 to 2015

Notes: The histogram shows the average GDP p.c. growth for the years 2001 to 2015 and the relevant destination countries. Data comes from the World Bank national accounts data, and OECD National Accounts data files.



**Table A.4:** Data sources for alternative network measures

<b>Data</b>	<b>Years</b>	<b># Fil.</b>	<b>Description</b>	<b>Information available</b>
U.S., Social Security Applications and Claims Index	1936-2007	132,599	Information filed with the Social Security Administration through the application or claims process	Name, date and place of birth, citizenship, sex
California, Passenger and Crew Lists	1882-1959	60,157	Passenger and crew lists of ships and some airplanes arriving in California.	Name, age, birth date and place, gender, nationality, last residence, port of departure and arrival, date of arrival
New York, Passenger and Crew Lists	1900-1959	5,643	Passenger lists of ships arriving from foreign ports at the port of New York.	Name, age, birth date and place, gender, nationality, last residence, port of departure and arrival, date of arrival
Honolulu, Hawaii, Passenger and Crew Lists	1820-1957	222,154	Passenger arrival and departure lists for Honolulu by ship and airplane	Name, age, birth place, gender, nationality, occupation, last residence, ultimate destination, date of arrival
Florida, Passenger List	1898-1963	1,996	Passenger lists of ships and airplanes arriving from foreign ports at various Florida ports.	Name, age, birth place, gender, nationality, port of departure and arrival, date of arrival, final destination
Washington, Passenger and Crew Lists	1882-1965	32,323	Passenger and crew lists of ships arriving at ports in Washington state.	Name, age, birth date and place, gender, nationality, last residence, port of departure and arrival, date of arrival

Notes: # Fil. corresponds to the number of Filipinos in the data. Data is obtained from Ancestry.com.

### A.3 Empirical Analysis

**Table A.5:** Effect of typhoon exposure on migration, average and by gender

	Average	Males	Females
<b>Panel A: Average effect</b>			
$SSI_{t-1}$	0.93*** (0.19)	0.49*** (0.10)	0.44*** (0.13)
Rel. to mean dep. var (pct.)	1.6	2.1	1.2
<b>Panel B: Network effect</b>			
$SSI_{t-1}$	-0.72** (0.34)	-0.29* (0.17)	-0.43* (0.23)
$SSI_{t-1} \times \text{Network}_{2000}$	2.20*** (0.44)	1.04*** (0.23)	1.15*** (0.29)
Observations	24360	24360	24360
# Cluster	1624	1624	1624
Mean dependent variable	59.44	22.91	36.52

Notes: The analysis is based on individual migration data from CFO for the years 2001 to 2015 aggregated at municipality-level. Population data is from the Philippine Censuses and interpolated for the years without census. The dependent variable is yearly migration rate (in 1/1000 pct.). The regression includes municipality and year fixed effects. Standard errors are clustered at municipality-level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table A.6:** Effect of typhoon exposure on migration by age

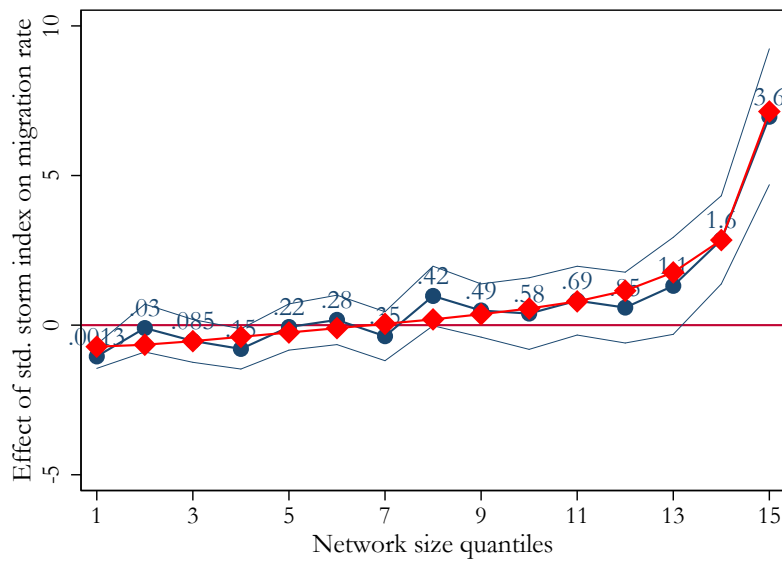
	Age 0-13	Age 14-21	Age 22-44	Age 45-64	Age 65-
<b>Panel A: Average effect</b>					
$SSI_{t-1}$	0.23*** (0.06)	0.12** (0.05)	0.41*** (0.12)	0.15* (0.08)	0.02 (0.05)
Rel. to mean dep. var (pct.)	3.5	1.5	1.5	1.2	0.4
<b>Panel B: Network effect</b>					
$SSI_{t-1}$	-0.28*** (0.10)	-0.10 (0.11)	-0.25 (0.16)	0.01 (0.11)	-0.10* (0.06)
$SSI_{t-1} \times \text{Network}_{2000}$	0.68*** (0.15)	0.29** (0.14)	0.89*** (0.20)	0.18 (0.15)	0.16** (0.07)
Observations	24360	24360	24360	24360	24360
# Cluster	1624	1624	1624	1624	1624
Mean dependent variable	6.65	7.5	27.93	11.95	5.38

Notes: The analysis is based on individual migration data from CFO for the years 2001 to 2015 aggregated at municipality-level. Population data is from the Philippine Censuses and interpolated for the years without census. The dependend variable is yearly migration rate (in 1/1000 pct.). The regression includes municipality and year fixed effects. Standard errors are clustered at municipality-level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table A.7:** Effect of typhoon exposure on migration by education

	Less than primary	Primary	Secondary	College
<b>Panel A: Average effect</b>				
$SSI_{t-1}$	0.32*** (0.11)	0.11 (0.10)	0.01 (0.08)	0.06 (0.10)
Rel. to mean dep. var (pct.)	2.4	1.8	0.2	0.9
<b>Panel B: Network effect</b>				
$SSI_{t-1}$	-0.12 (0.17)	0.21* (0.12)	0.04 (0.10)	-0.28*** (0.11)
$SSI_{t-1} \times \text{Network}_{2000}$	0.59*** (0.22)	-0.14 (0.14)	-0.04 (0.12)	0.46*** (0.11)
Observations	24360	24360	24360	24360
# Cluster	1624	1624	1624	1624
Mean dependent variable	13.41	6.16	6.46	7.17

Notes: The analysis is based on individual migration data from CFO for the years 2001 to 2015 aggregated at municipality-level. Population data is from the Philippine Censuses and interpolated for the years without census. The dependend variable is yearly migration rate (in 1/1000 pct.). The regression includes municipality and year fixed effects. Standard errors are clustered at municipality-level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Figure A.10:** Effect of storms on migration rate by network quantiles

Notes: The analysis is based on individual migration data from CFO for the years 2001 to 2015 aggregated at municipality-level. The x-axis displays 15 network size quantiles. The y-axis the effect on the yearly migration rate (in 1/1000 pct.). The regression includes municipality and year fixed effects. Standard errors are clustered at municipality-level. The thin blue lines show the 95 confidence interval. The blue dots show the interaction term between the network size quantile and the standardized storm index. The red diamonds show the effect calculated at the mean migration rate for the different quantile from a regressing of the migration rate on the continuous network size measure (as done in the baseline specification).

**Table A.8:** Effect of typhoon exposure on migration by category

	Children	Parents	Spouses	Siblings	Spouses of foreign.	Emp. migrants
<b>Panel A: Average effect</b>						
$SSI_{t-1}$	0.35*** (0.08)	0.07 (0.06)	0.25*** (0.09)	-0.01 (0.03)	-0.12** (0.06)	0.08** (0.04)
Rel. to mean dep. var (pct.)	2.5	0.9	1.7	-0.4	-1.1	2.8
<b>Panel B: Network effect</b>						
$SSI_{t-1}$	-0.38** (0.18)	-0.18** (0.08)	-0.47*** (0.12)	0.09** (0.05)	-0.05 (0.07)	0.12*** (0.04)
$SSI_{t-1} \times \text{Network}_{2000}$	0.97*** (0.25)	0.34*** (0.11)	0.95*** (0.13)	-0.13* (0.07)	-0.10* (0.06)	-0.04 (0.05)
Observations	24360	24360	24360	24360	24360	24360
# Cluster	1624	1624	1624	1624	1624	1624
Mean dependent variable	14.15	8.5	14.94	2.41	10.76	2.96

Notes: The analysis is based on individual migration data from CFO for the years 2001 to 2015 aggregated at municipality-level. Population data is from the Philippine Censuses and interpolated for the years without census. The dependent variable is yearly migration rate (in 1/1000 pct.). The regression includes municipality and year fixed effects. Standard errors are clustered at municipality-level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table A.9:** Effect heterogeneity by municipality characteristics

		Municipality heterogeneities		
	Average effect	Network	Population	Income
<b>Average effect</b>				
SSI <sub>t-1</sub>	0.93*** (0.19)			
<b>Network size</b>				
Small × SSI <sub>t-1</sub>		-0.47** (0.20)		
Medium × SSI <sub>t-1</sub>		0.29 (0.23)		
Large × SSI <sub>t-1</sub>		2.25*** (0.37)		
<b>Population size</b>				
Small × SSI <sub>t-1</sub>			0.58 (0.37)	
Medium × SSI <sub>t-1</sub>			0.88*** (0.29)	
Large × SSI <sub>t-1</sub>			1.43*** (0.26)	
<b>Income level</b>				
Low × SSI <sub>t-1</sub>				0.92*** (0.30)
Medium × SSI <sub>t-1</sub>				0.62** (0.27)
High × SSI <sub>t-1</sub>				1.48** (0.62)
Observations	24360	24360	24360	19575
# Cluster	1624	1624	1624	1305
Mean dependent variable	59.44	59.436	59.436	59.436

Notes: The table displays coefficients from regressing the yearly migration rate (in 1/1000 pct.) on the standardized storm index in year t-1 interacted with municipality characteristics. Coefficients are displayed for the average effect and for separate regressions for heterogeneities with respect to network size, population and income level. All regressions include municipality and year fixed effects. Standard errors are clustered at municipality-level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table A.10:** Effect heterogeneity by municipality characteristics (linear)

	Average effect	Municipality heterogeneities			
		Network	Population	Income	All
$SSI_{t-1}$	0.93*** (0.19)	-0.72** (0.34)	-2.72 (3.37)	0.73*** (0.24)	-2.55 (4.46)
$SSI_{t-1} \times \text{Network}_{2000}$		2.20*** (0.44)			2.23*** (0.48)
$SSI_{t-1} \times \text{Population (log)}$			0.35 (0.32)		0.12 (0.42)
$SSI_{t-1} \times \text{High income mun.}$				0.26 (0.34)	0.76* (0.42)
Observations	24360	24360	24360	24075	24075
# Cluster	1624	1624	1624	1605	1605
Mean dependent variable	60.06	60.06	60.06	60.06	60.06

Notes: The analysis is based on individual migration data from CFO for the years 2001 to 2015 aggregated at municipality-level. Population data is from the Philippine Censuses and interpolated for the years without census. The dependend variable is yearly migration rate (in 1/1000 pct.). The regression includes municipality and year fixed effects. Standard errors are clustered at municipality-level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table A.11:** Effect of typhoon exposure on migration, robustness

	Weighting and time trends				Current muni.	Prov. cluster	Conley HAC
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<b>Panel A: Average effect</b>							
$SSI_{t-1}$	0.93*** (0.19)	0.41** (0.20)	1.24*** (0.23)	0.73*** (0.23)	0.91*** (0.18)	0.93*** (0.25)	0.93** (0.37)
Rel. to mean dep. var (Pct.)	1.6	0.7	2.1	1.2	1.5	1.6	1.6
<b>Panel B: Network effect</b>							
$SSI_{t-1}$	-0.72** (0.34)	-1.07*** (0.28)	-0.76** (0.34)	-1.52*** (0.27)	-1.40*** (0.31)	-0.72* (0.43)	-0.72 (0.48)
$SSI_{t-1} \times \text{Network}_{2000}$	2.20*** (0.44)	1.98*** (0.36)	2.09*** (0.34)	2.36*** (0.30)	3.06*** (0.39)	2.20*** (0.52)	2.20*** (0.61)
Observations	24360	24360	24360	24360	24360	24360	24360
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mun-Year trends	No	Yes	No	Yes	No	No	No
Population weights	No	No	Yes	Yes	No	No	No
# Cluster	1624	1624	1624	1624	1624	87	
Mean dependent variable	59.44	59.44	59.44	59.44	59.44	59.44	59.44

Notes: The analysis is based on individual migration data from CFO for the years 2001 to 2015 aggregated at municipality-level. Population data is from the Philippine Censuses and interpolated for the years without census. The dependent variable is yearly migration rate (in Pct.). The regression includes municipality and year fixed effects. In column (5) individual migration data is aggregated using the current municipality. In the other columns the aggregation is based on the birth place municipality. In column (1)-(5) standard errors are clustered at municipality-level, in column (6) on province level and in column (7) standard errors are estimated using Conley spatial HAC. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .



**Table A.12:** Effect of heavy typhoons on migration

	(1)	(2)
<b>Panel A: Average effect</b>		
Heavy storm ( $SSI_{t-1} > 75\text{th quantile}$ )	3.02*** (0.51)	3.02*** (0.51)
Rel. to mean dep. var (Pct.)	5.1	5.1
<b>Panel B: Network effect</b>		
Heavy storm ( $SSI_{t-1} > 75\text{th quantile}$ )	0.08 (1.05)	0.03 (0.48)
Heavy storm ( $SSI_{t-1} > 75\text{th quantile}$ ) $\times$ Network <sub>2000</sub>	3.10** (1.28)	
Heavy storm ( $SSI_{t-1} > 75\text{th quantile}$ ) $\times$ Above median network size		4.59*** (0.89)
Observations	24360	24360
# Cluster	1624	1624
Mean dependent variable	59.44	59.44

Notes: The analysis is based on individual migration data from CFO for the years 2001 to 2015 aggregated at municipality-level. Population data is from the Philippine Censuses and interpolated for the years without census. The dependend variable is yearly migration rate (in 1/1000 pct.). The regression includes municipality and year fixed effects. Standard errors are clustered at municipality-level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table A.13:** Effect of typhoon exposure on migration, spillovers

	all	20 km	40 km	60 km	80 km	100 km
<b>Panel A: Average effect</b>						
Heavy storm	3.02*** (0.51)	3.63*** (0.53)	4.06*** (0.53)	4.50*** (0.54)	5.20*** (0.55)	5.52*** (0.56)
Rel. to mean dep. var (pct.)	5.1	6.1	6.8	7.6	8.7	9.3
<b>Panel B: Network effect</b>						
Heavy storm	0.08 (1.05)	0.15 (1.11)	0.24 (1.09)	0.60 (1.12)	0.72 (0.98)	0.88 (1.00)
Heavy storm $\times$ Network <sub>2000</sub>	3.10** (1.28)	3.73*** (1.39)	4.14*** (1.38)	4.28*** (1.43)	4.99*** (1.26)	5.24*** (1.29)
Observations	24360	23764	23070	22355	21679	21131
# Cluster	1624	1624	1624	1624	1624	1624
Mean dependent variable	59.44	59.44	59.44	59.44	59.44	59.44

Notes: The analysis is based on individual migration data from CFO for the years 2001 to 2015 aggregated at municipality-level. Population data is from the Philippine Censuses and interpolated for the years without census. The dependend variable is yearly migration rate (in 1/1000 pct.). The regression includes municipality and year fixed effects. Standard errors are clustered at municipality-level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table A.14:** Correlation between different network measures

	Network USA 2000	Network social security index	Network passenger lists
Network USA 2000	1.00	0.54	0.28
Network social security index	0.54	1.00	0.76
Network passenger lists	0.28	0.76	1.00

Notes: The table shows pairwise correlation coefficients of the different network measures aggregated at municipality-level. *Network USA 2000* is based on flow migration data from CFO for the years 1988 to 2000. *Network social security index* is based on US Social Security Applications and Claims data that covers information about individuals who passed away in the US before 2007. *Network passenger lists* is based on data from several historical passenger lists for immigrants to the US from 1820 to 1965.

**Table A.15:** Effect of typhoon exposure on migration, alternative network measures

	Main network measure		Alternative network measures			
	USA 2000	USA 2000	SSI	SSI	PL	PL
SSI <sub>t-1</sub>	-0.25 (0.23)	-0.69*** (0.22)	0.31* (0.16)	0.27 (0.19)	0.47*** (0.15)	0.52*** (0.16)
SSI <sub>t-1</sub> × Network size	1.48*** (0.44)	2.48*** (0.43)	1.42* (0.81)	2.16** (1.06)	0.22 (0.17)	0.31 (0.24)
Observations	24360	24360	24360	24360	24360	24360
# Cluster	1624	1624	1624	1624	1624	1624
Mean dependent variable	37.75	37.75	37.75	37.75	37.75	37.75
Mun-Year trends	No	Yes	No	Yes	No	Yes

Notes: The analysis is based on individual migration data from CFO for the years 2001 to 2015 aggregated at municipality-level. Population data is from the Philippine Censuses and interpolated for the years without census. The dependent variable is yearly migration rate (in 1/1000 pct.). The network variable is measured in different ways. For columns (1) and (2) flow data for the migrants from the Philippines to the US from the Commission on Filipinos Overseas cumulated for the years 1988 to 2000, divided by a municipalities population and measured in percent. For columns (3) and (4) data from the U.S., Social Security Applications and Claims Index and for columns (3) and (4) data from US passenger lists are used to calculate the network measure following the same method as described above. Standard errors are clustered at municipality-level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

## A.4 Mechanisms

**Table A.16:** Effect of typhoon exposure on migration, destination country specific analysis

	(1)	(2)
SSI <sub><i>t</i>-1</sub>	0.05*** (0.01)	-0.02** (0.01)
SSI <sub><i>t</i>-1</sub> × Network <sub>2000</sub>		1.90*** (0.32)
Observations	487200	487200
# Cluster	1624	1624
Mean dependent variable	2.99	2.99
Rel. to mean dep. var (Pct.)	1.6	

Notes: The analysis is based on individual migration data from CFO for the years 2001 to 2015 aggregated at municipality and country of destination level. Population data is from the Philippine Censuses and interpolated for the years without census. The dependent variable is yearly migration rate (in 1/1000 pct.). The regression includes municipality-destination country and year fixed effects. Standard errors are clustered at municipality-level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table A.17:** The role of policy restrictiveness, mean policy restrictiveness index

	Family			Labor		
	Family members	Quotas	Financial requirem.	Tests	Job offer	Funds
$SSI_{t-1}$	-0.05*** (0.01)	-0.07*** (0.01)	-0.14*** (0.01)	0.01 (0.02)	0.00 (0.02)	-0.07*** (0.01)
$SSI_{t-1} \times \text{Network}_{2000}$	6.93*** (0.77)	7.18*** (0.77)	11.62*** (1.50)	-5.92*** (1.01)	-12.73*** (1.87)	-1.04** (0.52)
$SSI_{t-1} \times \text{PRI}_{\text{mean}}$	-0.00** (0.00)	0.00 (0.00)	0.00*** (0.00)	-0.00*** (0.00)	-0.00** (0.00)	0.00*** (0.00)
$SSI_{t-1} \times \text{Network}_{2000} \times \text{PRI}_{\text{mean}}$	-0.26*** (0.04)	-0.11*** (0.02)	-0.14*** (0.02)	0.31*** (0.04)	0.27*** (0.03)	0.33*** (0.04)
Observations	462840	462840	462840	462840	462840	414120
# Cluster	1624	1624	1624	1624	1624	1624
Mean dependent variable	2.99	2.99	2.99	2.99	2.99	2.99

Notes: The analysis is based on individual migration data from CFO for the years 2001 to 2015 aggregated at municipality and destination country level. Population data is from the Philippine Censuses and interpolated for the years without census. Data for the Policy Restrictiveness Index is from the Immigration Policies in Comparison (IMPIC) database. The policy restrictiveness indices ( $\text{PRI}_{\text{mean}}$ ) are the average PRIs from 2001 to 2010 (years for which data is available) and are measured in percent with zero being not restrictive and 100 being very restrictive. The dependend variable is destination country specific yearly migration rate (in 1/1000 pct.). The regression includes municipality-destination country and year fixed effects. Standard errors are clustered at municipality-level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table A.18:** The role of policy restrictiveness, policy restrictiveness index in 2000

	Family			Labor		
	Family members	Quotas	Financial requirem.	Tests	Job offer	Funds
$SSI_{t-1}$	-0.06*** (0.01)	-0.07*** (0.01)	-0.12*** (0.01)	0.03 (0.02)	0.04* (0.02)	-0.06*** (0.01)
$SSI_{t-1} \times \text{Network}_{2000}$	7.90*** (0.91)	7.18*** (0.77)	11.47*** (1.49)	-8.12*** (1.30)	-12.85*** (1.87)	-1.04** (0.52)
$SSI_{t-1} \times \text{PRI}_{2000}$	-0.00 (0.00)	0.00 (0.00)	0.00*** (0.00)	-0.00*** (0.00)	-0.00*** (0.00)	0.00** (0.00)
$SSI_{t-1} \times \text{Network}_{2000} \times \text{PRI}_{2000}$	-0.31*** (0.05)	-0.11*** (0.02)	-0.14*** (0.02)	0.39*** (0.05)	0.27*** (0.03)	0.33*** (0.04)
Observations	462840	462840	462840	462840	462840	414120
# Cluster	1624	1624	1624	1624	1624	1624
Mean dependent variable	2.99	2.99	2.99	2.99	2.99	2.99

Notes: The analysis is based on individual migration data from CFO for the years 2001 to 2015 aggregated at municipality and destination country level. Population data is from the Philippine Censuses and interpolated for the years without census. Data for the Policy Restrictiveness Index is from the Immigration Policies in Comparison (IMPIC) database. The policy restrictiveness indices ( $\text{PRI}_{2000}$ ) are fixed in the year 2000 and measured in percent with zero being not restrictive and 100 being very restrictive. The dependend variable is destination country specific yearly migration rate (in 1/1000 pct.). The regression includes municipality-destination country and year fixed effects. Standard errors are clustered at municipality-level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table A.19:** Effect of past GDP per capita growth on employment and wage income

	<b>Employment</b>		<b>Income (log)</b>	
	(1)	(2)	(3)	(4)
Average GDP p.c. growth (t-5 to t-1)	0.004** (0.002)		0.004 (0.004)	
Average GDP p.c. growth (t-10 to t-1)		0.006*** (0.002)		0.014*** (0.004)
Observations	74839	74839	60510	60510

Notes: The analysis is based on Filipino immigrants in the US who are likely to be in the workforce, i.e., between 23 and 60 years old. Data comes from the 2000 US Census and 2001 to 2017 American Community Survey. The regression includes controls for gender, age, age to the square, years of schooling, years of schooling to the square, as well as state, year and years in the US fixed effects. Employment is a dummy variable that is one if the individual is employed. Income (log) is the log of real wage income conditional on being employed. Identification comes from variation in average state-level GDP p.c. growth in the last five (ten) years previous to observation. Standard errors are clustered on state-year level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .



## Chapter 2

# Immigrating into a Recession<sup>\*</sup>

### Abstract

We analyze how economic conditions at the time of arrival shape the economic integration of family migrants in the US. Our identification strategy exploits years-long waiting times for visa that decouple the migration decision from economic conditions at the time of arrival. We find that initial economic conditions have considerable consequences for the integration of family migrants. A one pp higher initial unemployment rate decreases wage income by four percent in the short run and by 2.5 percent in the longer run. This is the result of substantial occupational downgrading. Family migrants who immigrate into a recession draw on ethnic networks to mitigate the negative labor market effects. They are also more likely to reside with family members, reducing their geographical mobility.

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<sup>\*</sup> This chapter is based on joint work with Toman Barsbai and Andreas Steinmayr.

## 2.1 Introduction

Family-sponsored visas are the primary entryway for permanent immigrants to the OECD. In 2015, family migrants accounted for 48 percent of all permanent immigrant arrivals in the OECD. By contrast, labor migrants accounted for only 16 percent. This pattern also holds for the US. In 2015, 65 percent of all persons obtaining lawful permanent resident status in the US were family-based migrants. Only 14 percent were employment-based migrants.<sup>1</sup> Nevertheless, family migrants have received limited attention in economics.

Two key features characterize the system of family-sponsored visas in the US. First, as a result of yearly caps on the number of available visas and excess demand, it usually takes several years until a family-sponsored visa is granted. In addition, once family members obtain their visas, they must move to the US within a period of six months. Hence, the macroeconomic conditions family migrants face at arrival in the US are beyond their control. Some family migrants happen to immigrate into a boom, whereas others happen to immigrate into a recession. Second, as their visas are not sponsored by an employer, family migrants typically arrive without a job. These two features make the economic integration of family migrants potentially susceptible to initial labor market conditions.

In this paper, we analyze how unemployment rates at the time of arrival in the US shape the economic integration of immigrants over a period of ten years. We introduce a new identification strategy that exploits the inability of family migrants to synchronize their arrival with labor market conditions in the US. Our focus is on key labor market outcomes, in particular, employment and wage income. We also provide evidence on the underlying mechanisms and coping strategies.

Economic theory suggests that entering the labor market in a recession increases the odds of unemployment and job-skills mismatch. Due to lower levels of destination-specific human capital, immigrants might also be pushed to enter occupations with larger ethnic networks. These jobs may provide fewer opportunities to accumulate destination-specific human capital and experience, thus limiting upward mobility. In addition, future employers may perceive past labor market outcomes as a signal of productivity. Initially, unemployed or mismatched immigrants may thus face persistently lower wages. We thus hypothesize that immigrating into a recession is associated with worse labor market outcomes, even in the longer run.

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<sup>1</sup> Figures for the OECD come from the 2017 International Migration Outlook and exclude migration movements within areas of free circulation (mainly within the EU). Figures for the US come from the 2015 Yearbook of Immigration Statistics. Humanitarian migrants (refugees and asylum seekers) and winners of the diversity lottery in the US account for most of the remaining share.



We base our empirical analysis on cross-sectional data from the American Community Survey and the US census for the period 2000-2016. The data provides information on the year of immigration. We are thus able to analyze the effect of state-level unemployment rates in the year of arrival on immigrants' labor market outcomes in the year of observation. Our econometric specification includes years-since-migration, state, year-of-observation, and year-of-immigration fixed effects and exploits variation in initial unemployment rates within states over time. This specification allows us to control for the general path of economic integration over time, persistent differences in economic conditions or immigrant characteristics across states, nation-wide economic conditions at the time of observation, and changes in the characteristics of immigrant cohorts.

The key challenge for identification is that migration decisions are endogenous to economic conditions. If the characteristics of immigrants to a specific state differ between good and bad economic times, observed differences in economic integration may be due to differences in immigrant characteristics, not differences in initial economic conditions. As argued above, family migrants cannot choose their date of immigration based on economic conditions. In addition, family migrants typically join their sponsor's household in the US. Hence, family migrants do not choose the US state based on economic conditions either. As a result, the local economic conditions family migrants face at arrival are exogenous. We offer support for our identifying assumption by showing that unemployment rates in the year of arrival cannot predict the size and composition of inflows of family migrants to US states. We also show that selective return migration is unlikely to bias our results. In general, rates of return migration are very low for family migrants. We also show that survival rates of migrants in our sample do not systematically differ by year of arrival and hence different initial economic conditions.

Currently available datasets do not provide information on the visa type. We are thus not able to directly identify family migrants. In our main analysis, we therefore restrict the sample to working-age immigrants from countries for which family-based migration is the dominant mode of migration to the US. We also offer alternative strategies that identify (i) family migrants from the Philippines based on administrative data from the Philippine government and (ii) immigrant spouses with waiting times based on the timing of immigration and marriage.

We have three main findings. First, immigrating into a recession substantially worsens labor market outcomes, even in the longer run. A one pp higher unemployment rate at the time of arrival has only a small and short-lasting effect on employment rates. However, it decreases real wage income in the first four years by about four percent,

with slow convergence to a persistent negative effect of about 2.5 percent afterward. The average family migrant thus experiences a cumulative loss of wage income of about USD 25,000 over a period of ten years from immigrating into a recession, which roughly corresponds to a three pp increase in the unemployment rate. Second, the negative effect on wage income is the result of a combination of occupational downgrading, lower hourly wages, and a decrease in working hours. Third, family migrants rely on ethnic and family networks to cope with adverse economic conditions. Migrants who arrive at times of high unemployment use ethnic networks to mitigate the negative labor market effects. They are also more likely to reside with family members, most likely the sponsor of their visa. The immobile support received from the family, however, may reduce the geographic mobility of migrants. Indeed, we find that migrants who arrive at times of high unemployment do not increase their geographical mobility. The resulting job search frictions could provide a potential explanation for the observed persistence of the effects.

Our paper builds on and contributes to two strands of literature. One strand of the literature has examined how immigrant earnings evolve after their arrival in the destination country and whether immigrant earnings assimilate to native earnings over time (starting with the seminal papers by Chiswick (1978) and Borjas (1985)).<sup>2</sup>

The other strand of the literature we relate to has studied how entering the labor market in a recession affects labor market outcomes. A few studies have focused on immigrants. Chiswick et al. (1997), using data from the US Current Population Survey for the 1980s, find that immigrants who arrive in a recession have lower employment rates initially but quickly catch up with natives. In contrast, Chiswick and Miller (2002), using data from the 1990 US Census, find that immigrants who arrive in a recession have substantially lower earnings and take about 30 years to close this earning gap. These studies, however, do not address the potential endogeneity of migration decisions. Differences in observed labor market outcomes may therefore be due to differences in immigrant characteristics, not differences in initial economic conditions.

Most closely related to our paper, Åslund and Rooth (2007) explicitly address this issue. They exploit a placement policy that exogenously assigns refugees to initial locations in Sweden. They find that arriving in a recession reduces refugees earnings and employment rates for at least ten years. Godøy (2017) and Mask (2018) also exploit refugee placement policies and report similar, but less long-lasting effects for refugees in Norway and the US.

A number of studies have also investigated the labor market effects of graduating in a recession. Using data on Norwegian secondary school graduates from the 1990s,

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<sup>2</sup> For a recent review, see Duleep (2015).

Raaum and Røed (2006) find that those who graduate in a recession are less likely to be employed during the first ten years of their work careers. Using data on US college graduates from the 1980s, Kahn (2010) shows that those who graduate in a recession persistently earn lower wage incomes and work in lower-level occupations, but do not see changes in their working hours. Using data on Canadian college graduates from the 1980s and early 1990s, Oreopoulos et al. (2012) document similar effects. More recently, Altonji et al. (2016), using data on US college graduates from the period 1974-2011, find that those who graduate in a recession see a substantial reduction in initial earnings through less full-time work and lower wages. They document, however, only small persistent effects on wages.

A few studies have gone beyond graduates and extend the set of workers under study to include school leavers at all levels. Kwon et al. (2010), using data on white-collar workers from Sweden for the period 1970-1990, show that workers who enter the labor market during a boom earn higher wages and are promoted more quickly. Brunner and Kuhn (2014), using data on male workers entering the Austrian labor market between 1978 and 2000, find that high unemployment rates at the time of labor market entry have a persistent negative effect on wage income. This effect is more pronounced for blue-collar workers who may be permanently locked into low-paying jobs. Most recently, Schwandt and von Wachter (2019) show that entering the labor market at times of high unemployment substantially reduces earnings. The effect is driven by a reduction in working hours and hourly wages and persists for about ten years.

We contribute to these two strands of literature in a number of ways. First, we provide the first evidence on the economic integration of family migrants who constitute a very relevant population group in the US and the OECD in general. Second, we introduce a novel identification strategy and show that initial economic conditions can generate substantial heterogeneity in the evolution of earnings across immigrant cohorts. Third, we broaden the evidence that labor market outcomes are path-dependent and not only depend on contemporaneous economic conditions (e.g., see Beaudry and DiNardo, 1991). It is not clear a priori that previous findings for native labor market entrants and refugees also apply to family migrants. Family migrants may enter different segments of the labor market and can draw on established networks that may cushion the effects of initial economic conditions. Fourth, we show that adverse economic conditions at the time of arrival constitute a key mechanism for explaining widely documented immigrant-specific phenomena such as downgrading and reliance on family and ethnic networks.

## 2.2 Theoretical Considerations

In this section, we briefly explore potential mechanisms through which initial economic conditions potentially affect the economic integration of immigrants. Recessions affect the number and types of initial job opportunities available on the labor market. Entering the labor market in a recession hence likely increases the odds of being unemployed or experiencing job mismatching with the associated lower wages (Bowlus, 1995; McLaughlin and Bils, 2001; Devereux, 2002). This may be particularly true for immigrants, as their labor market outcomes are generally more responsive to business cycle fluctuations than those of natives (Dustmann et al., 2010; Orrenius and Zavodny, 2010). If future employers perceive past spells of unemployment or low wages as a signal of low productivity, initially unemployed or mismatched individuals may then face persistently lower wages (Jacobson et al., 1993; Arulampalam, 2001; Gregg, 2001; Gregory and Jukes, 2001; Couch and Placzek, 2010; Kroft et al., 2013).

The scarring effect is likely more pronounced for immigrants than for natives. Initial unemployment and job mismatching make them less likely to accumulate the optimal destination-country specific human capital and experience. Given the limited transferability of human capital and experience from the origin to the destination country (e.g., Friedberg, 2000), it may thus be more difficult for immigrants than natives to signal their idiosyncratic productivity to future employers. Similarly, if immigrants initially enter the ethnic economy and accumulate skills that are specific to the ethnic economy, they may be more likely to remain in it and potentially face limited upward mobility (similar to the argument made by Borjas (1992)).

However, adverse economic conditions at arrival do not need to have a persistent effect on the labor market outcomes of immigrants. Immigrants may simply take more time to complete the job-matching process. With diminishing marginal returns to the optimal labor market experience, immigrants may be able to recover from initial shocks over time. The ability to do so depends on the presence of search frictions and immigrants occupational, sectoral, and geographical mobility (on the importance of mobility, see Topel and Ward, 1992). On the one hand, due to the scarring effect and discrimination on the labor market, immigrants may face more search frictions than natives. On the other hand, due to lower degrees of attachment, immigrants may also have lower moving costs and therefore be more mobile than natives (Green, 1999; Braun and Kvasnicka, 2014; and Cadena and Kovak, 2016). The mobility of family migrants, however, might be lower as they initially move to their sponsor's household and might depend on the support of family members. From a theoretical perspective, it is thus not clear how persistent the negative labor market effects of immigrating into a recession are.

## 2.3 The System of Family-sponsored Immigration to the US

The Immigration Act of 1990 established the current system of legal permanent immigration to the US. There are four main pathways of permanent immigration: family-sponsored immigration, employment-based immigration, the Diversity Immigrant Visa program, and admission on humanitarian grounds through refugee and asylum programs. US immigration law has historically emphasized family reunification, which remains the most common legal basis for immigration to the US.<sup>3</sup>

Family migrants to the US require a sponsor who provides legal entitlement to a visa. Family members residing in the US as a US citizen or a lawful permanent resident (LPR or green-card holder) can act as sponsors. Sponsors need to prove that they can support their own family and the sponsored family member at an income level at or above 125 percent of the federal poverty level. They also need to sign an affidavit of financial support, which legally obliges them to support sponsored family members for ten years or until they become a US citizen (Kandel, 2018b).

The Immigration and Nationality Act of 1965 provides several broad classes of admission for family members to gain LPR status. The resulting admission categories depend on the relationship between the sponsor and the family member, the age and marital status of the family member, and on whether the sponsor is a US citizen or a LPR (see Table 2.1). Importantly, there are caps on the number of visas available in each category per year. The only exception are immediate relatives of US citizens, i.e., spouses, parents, and unmarried children under the age of 21 years. They are not subject to numerical limitations.

The Immigration Act of 1990 caps the total number of immigrants to come to the US as an LPR at 675,000 per year. This number includes 480,000 family-sponsored immigrants, 140,000 employment-based immigrants, and 55,000 diversity immigrants. To account for the fact that immediate relatives of US citizens are not subject to numerical limitations, the annual cap of 480,000 family-sponsored immigrants is adjusted in the following way:

- 480,000 (annual total cap on family-sponsored immigrants)
- number of immediate relatives granted LPR status in the prior year
- number of aliens paroled into the US for at least a year in the prior year
- + number of unused employment-based visas from the prior year

The minimum adjusted number of family-sponsored immigrants, however, is fixed at 226,000 per year. As the number of immediate relatives granted LPR status has exceeded 254,000 every year, the annual cap for other family-sponsored immigrants

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<sup>3</sup> There are a few other pathways to Lawful Permanent Resident status, but they are quantitatively not important.

**Table 2.1:** Family-sponsored admission categories

Category	Sponsor	Family members	Visas
<b>IR</b>	US citizen	Spouses, parents and unmarried children under 21	unlimited
<b>F1</b>	US citizen	Unmarried sons and daughters and their minor children	23,400
<b>F2A</b>	LPR	Spouses and minor children	87,900
<b>F2B</b>	LPR	Unmarried sons and daughters	26,300
<b>F3</b>	US citizen	Married sons and daughters	23,400
<b>F4</b>	US citizen	Siblings and their minor children	65,000

Source: Congressional Research Service, summary of INA §203(a) and §204; 8 USC. §1153.

Note: Unused visas from the previous year can partially increase the number of visas in individual categories. For more details see CSR report R43145.

has effectively remained at the minimum of 226,000 for the past two decades (Kandel, 2018a). Table 2.1 shows the resulting number of family visas available for the different categories. In addition, per-country ceilings regulate that citizens from a single country cannot account for more than seven percent of all available visas.<sup>4</sup>

Demand for family-based migration far exceeds the number of available visas per year. As a result, the caps have created a large backlog of individuals whose LPR visa petitions have been approved by the US Citizenship and Immigration Services (USCIS) but for whom no visas are available. As of November 1, 2017, 3.95 million approved LPR visa petitions in the family track were pending and waiting for visa processing (Kandel, 2018a). Within admission categories, the Department of State processes visa applications in the order in which petitions were filed. Visa applicants therefore typically wait for several years before they receive their visa.

The Department of State regularly publishes the waiting times for those applicants who are currently invited for visa processing in its monthly Visa Bulletin. Figure 2.1 shows the waiting times, defined as the time between filing a petition and being invited for visa processing, for different categories of family migrants for the period 1990-2016. For example, in 2005, family migrants receiving F1 visas (unmarried sons and daughters of US citizens) or F2A visas (spouses and minor children of LPRs) had been in the queue for about four years, those receiving F2B visas (unmarried sons and daughters of LPRs) for about eight years, those receiving F3 visas (married sons and daughters of US citizens) for about six years, and those receiving F4 visas

<sup>4</sup> Exceptions are possible for category F2A and some employment-based visas. For more details, see Kandel (2018a).

(siblings of US citizens and their minor children) for about twelve years. Due to the binding per-country ceilings, applicants from the largest countries of origin, Mexico, China, India, and the Philippines, have different and mostly longer waiting times (see Figure B.1 in the appendix). Across admission categories and countries of origin, the average applicant waited for about ten years for a numerically limited visa during the period 1990-2016.

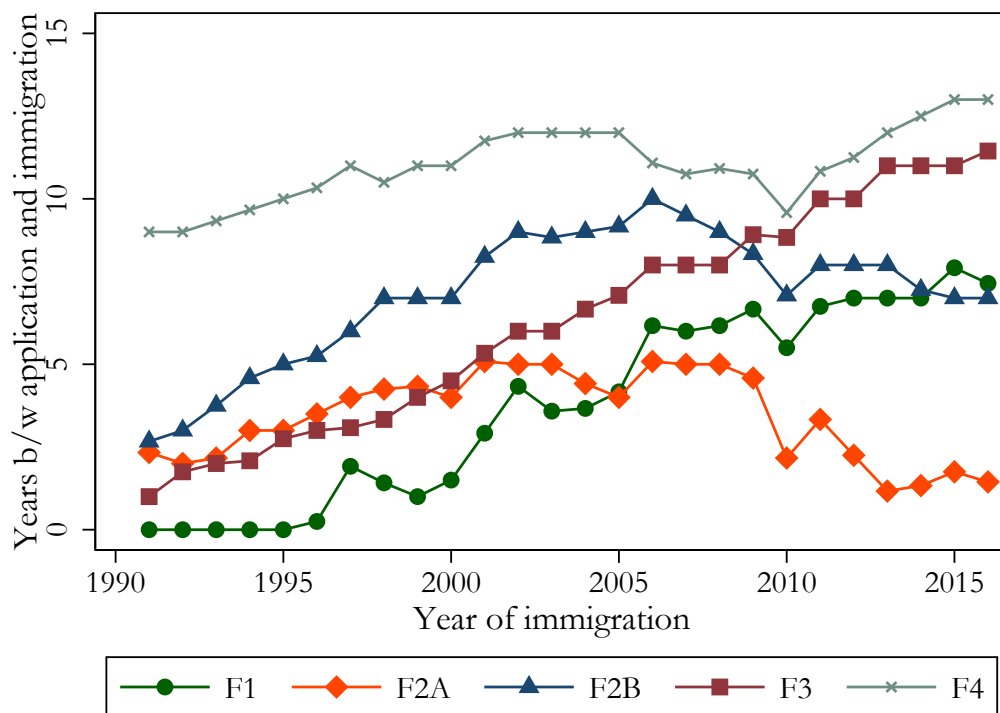
Prospective family migrants do not only face long waiting times, they also face considerable uncertainty about the length of waiting times. At the time of filing their petition, they only know the waiting times of applicants who have just been invited for visa processing in each admission category. Their effective waiting times, however, may well differ depending on how many other prospective migrants from the same and other countries have filed petitions in the same admission category. Indeed, as Figure 2.1 shows, waiting times have not been constant over time. They have considerably increased for most admission categories since the 1990s.

Once family members obtain their visa, they have limited options to choose the date of departure. Visas are only valid for up to six months, so migrants have to enter the US within that period. Only after arriving in the US will they receive their LPR status.<sup>5</sup>

The combination of long waiting times, uncertainty about the length of waiting times, and the short validity of the visa forces family migrants to enter the US within a narrow time window that is beyond their control and not known a priori. Family migrants are thus not able to synchronize their arrival with labor market conditions in the US. Our identification strategy exploits this particular feature of the US immigration system to overcome the problem of endogenous migration decisions.

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<sup>5</sup> Some migrants also adjust their status while being in the US. However, adjustment of status is not common for family migrants. According to the 2015 Yearbook of Immigration Statistics, only 16,783 family migrants did so in 2015.

**Figure 2.1:** Waiting times for family migrants by admission category

Notes: The graph shows waiting times for unmarried sons and daughters of US citizens (F1), spouses and children of legal permanent residents (F2A), unmarried adult children of legal permanent residents (F2B), married sons and daughters of US citizens (F3) and brothers and sisters of US citizens (F4). Data source: U.S. Department of State, own calculations. Figure B.1 in the appendix shows waiting times for applicants from Mexico, China, India, and the Philippines, where the per-country ceiling is binding.



## 2.4 Data

We base our analysis on data from the 2000 US census and the American Community Survey (ACS) for the period 2000-2017, obtained via IPUMS. This data has two advantages over other potential data sources. First, the sample is large, even if we restrict it to immigrants with specific characteristics. Second, the data provides information on the year of immigration and country of birth as well as a wide range of labor market outcomes.<sup>6</sup>

Like other currently available datasets, however, our data does not provide information on the visa type. And the US census and the ACS sample all types of permanent residents as well as aliens who reside in the US on a temporary visa or irregularly. We are hence not able to directly identify family migrants. In our main analysis, we therefore restrict the sample to immigrants from countries for which family-based migration is the dominant mode of migration to the US. As part of our robustness checks, we also explore two alternative strategies. Both yield similar results.

To understand the relative importance of different modes of migration to the US, we use data from the Yearbook of Immigration Statistics. Published by the US Department of Homeland Security, the yearbooks provide information on the number of immigrants and aliens who are admitted to the US by admission category, country of origin, and year. We only consider categories that are likely to be sampled by the census and the ACS. They include all different types of LPRs (family-based, employment-based and others, including diversity migrants and admissions on humanitarian grounds), students and exchange visitors on temporary visas, and temporary workers. They exclude tourists, business travelers, and diplomats. For each country of origin, we then calculate the share of three different types of migrants distinguishing between family-based migrants, employment-based migrants, and all other migrants.

Figure 2.2 shows the dominant mode of migration to the US for different countries. We define the dominant mode to account for more than 50 percent of yearly admissions in each of the years 2005, 2010, and 2015. Doing so allows us to identify countries for which the dominant mode of migration is stable over time. We define countries as mixed when no mode of migration accounts for more than 50 percent in all three years.

Countries with predominantly family-based migration include the Philippines, Vietnam, Cambodia, Laos, Yemen, Guyana, and some small island states. Countries with predominantly employment-based migration mostly consist of OECD countries. They also include Argentina, South Africa, and India. Other modes of migration only

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<sup>6</sup> We do not use the 2010 US census as it was a short-form-only census and does not provide the relevant information.

dominate in a few countries in sub-Saharan Africa. All other countries have mixed modes of migration to the US.

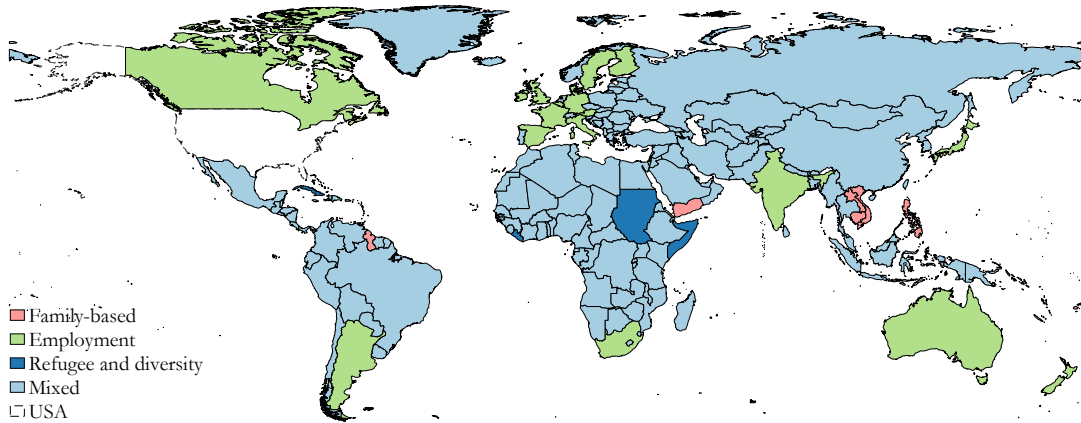
Table B.1 in the appendix shows that this definition of country types indeed captures meaningful differences in the composition of migrants. In 2015, family migrants accounted for 68 percent of migrants from countries with predominantly family-based migration. With 3 percent and 23 percent, the share of family migrants was much lower for countries with predominantly employment-based migration or mixed countries. Similarly, employment-based migrants accounted for 65 percent of migrants from countries with predominantly employment-based migration, but only for 9 percent and 21 percent of migrants from countries with predominantly family-based migration or mixed countries.

In the subsequent analysis, we restrict the sample to migrants from countries with predominantly family-based migration to the US. They are most likely to be family migrants. Individuals from the Philippines (55.7%) and Vietnam (30.9%) make up for the vast majority of the sample. Individuals from British Guyana (5.5%), Cambodia (2.3%), Laos (2.2%), Yemen (1%), and some small island states (2.4%) play a minor role in the sample. It is important to note, however, that these countries also send significant shares of non-family migrants and that many family migrants are immediate relatives who do not face long waiting times (see Table B.1 in the appendix). We revisit this issue in our robustness checks.

We also restrict the sample to individuals who were between 22 and 60 years old at the time of immigration and the time of observation. This age restriction focuses on individuals who are likely to become active in the labor market at arrival. In addition, it excludes minor children of US citizens who can be sponsored without waiting times (see Table 2.1). It also excludes young adults who immigrate as students or exchange visitors as well as parents of US citizens who immigrate after retirement. These groups do not face waiting times either. Finally, we also restrict the sample to migrants who have at most spent ten years in the US.

Table B.2 provides summary statistics of demographic characteristics and outcome variables for family migrants in column (1). For comparison, the table also shows the same statistics for migrants from the Philippines, the most important country of origin of family migrants in our sample, in column (2); employment-based migrants, in column (3); natives, i.e., US-born individuals, in column (4); and US-born college graduates in column (5). For all populations, we restrict the sample to individuals who were between 22 and 60 years old at the time of observation.

On average, family migrants immigrated at the age of 35 and faced a local unemployment rate of 6.14 percent at arrival. They are disproportionally female (63%)

**Figure 2.2:** Countries by dominant mode of migration to the United States

Notes: Data from the Yearbook of Immigration Statistics (Department of Homeland Security). The figure depicts countries by their dominant mode of migration to the United States in 2005. Countries identified as sending predominantly family migrants in 2005, 2010, and 2015 are Cambodia, Cape Verde, Dominican Republic, Fiji, Guyana, Haiti, Laos, Philippines, Saint Vincent and the Grenadines, Samoa, Tonga, Vietnam, Yemen.

and have on average 12.9 years of schooling. While this is seven months less than the average native, family migrants are more likely than natives to hold a college degree (39% vs. 30%). In comparison, employment-based migrants on average immigrated at the age of 31 and faced a local unemployment rate of 5.88 percent. Their sex ratio is more balanced (48% female), and they have considerably higher levels of schooling (15.8 years).

On average, family migrants are as old as natives at the time of observation (41 years). Employment migrants are considerably younger (36 years). The gap between age at immigration and observation is larger for employment migrants than for family migrants. This is consistent with higher rates of return migration for employment migrants, to which we will return later. In terms of labor market outcomes, family migrants do worse than natives. Their employment levels (70% vs. 75%), annual wage income conditional on being employed (USD 25k vs. USD 35k), real hourly wages conditional on being employed (USD 15 vs. USD 18), and occupational income scores (12 vs. 14), the log median wage income of a worker in the same occupation, are lower. Not surprisingly, employment-based migrants have a very strong labor market performance. They have substantially higher wage incomes (USD 53k), hourly wages (USD 27), and occupational income scores (19) than family-based migrants or natives.

Family migrants are not more likely to receive public welfare assistance than natives. This can be explained by the fact that family migrants have no access to welfare benefits in their first years after arrival. Finally, family migrants are more

likely to co-reside with their own siblings (6.5% vs. 4.6%) and much less likely to be household heads (32% vs. 53%) than natives. They are not more likely to have moved across states within the last year (2.4% vs. 2.7%). By contrast, employment-based migrants are generally more independent and mobile.

Migrants from the Philippines constitute the largest group of family migrants in our sample. Their demographic characteristics are comparable to all family migrants. However, they have significantly higher levels of schooling (14.7 vs. 12.9 years) and college graduation rates (61% vs. 39%). They also do better in the labor market than all family migrants, but still somewhat worse than natives.

By definition, college graduates have the highest level of education. They are also much younger on average and, given their younger age and limited experience, perform relatively well on the labor market. Out of the different groups, they are most likely to have moved across states within the last year (6.6%).

## 2.5 Empirical Approach

We estimate the following model to analyze how local unemployment rates at the time of arrival shape the economic integration of family migrants over time:

$$y_{i,s,t,m} = \alpha + \sum_{t=m+1}^{m+10} \beta_{(t-m)} UR_{s,m} + \gamma_{(t-m)} + \theta_s + \lambda_t + \chi_m + \mathbf{X}_{i,t}\Delta + \epsilon_{i,t} \quad (2.1)$$

where  $y_{i,s,t,m}$  measures the outcome of interest for family migrant  $i$  who immigrated in year  $m$  and was observed in year  $t$  in state  $s$ . The variable of interest is the state-level unemployment rate in the year of immigration  $UR_{s,m}$ . We use a fully flexible model and allow the effect of  $UR_{s,m}$  to vary with the number of years in the US ( $t - m$ ). We estimate the effect for the first ten years in the US. The coefficients  $\beta_{(t-m)}$  capture the effect of the unemployment rate in the year of immigration plus the weighted sum of the effects of unemployment rates in subsequent years. These parameters are of interest as they capture the overall effect of initial economic conditions for a normal evolution of state-level unemployment rates afterward (Oreopoulos et al., 2012).  $\gamma_{(t-m)}$ ,  $\theta_s$ ,  $\lambda_t$ , and  $\chi_m$  represent a full set of years-in-the-US, state, year-of-observation, and year-of-immigration fixed effects. They control for the general path of economic integration over time ( $\gamma_{(t-m)}$ ), persistent differences in economic conditions or immigrant characteristics across states ( $\theta_s$ ), nation-wide economic conditions at the time of observation ( $\lambda_t$ ), and nation-wide economic conditions at the time of immigration and changes in the characteristics of immigrant cohorts ( $\chi_m$ ).  $\mathbf{X}_{i,t}$  is a vector of migrant-level controls. It includes age, age squared, gender, years of schooling, and a full set of

country-of-origin dummies.  $\epsilon_{i,t}$  is an i.i.d error term.

We also use a second specification where we model the dynamic effects of the unemployment rate in the year of immigration with a fifth-order polynomial:

$$y_{i,s,t,m} = \alpha + \sum_{j=1}^5 \beta_j UR_{s,m}(t-m)^j + \gamma_{(t-m)} + \theta_s + \lambda_t + \chi_m + \mathbf{X}_{i,t}\Delta + \epsilon_{i,t} \quad (2.2)$$

The polynomial specification is less flexible and introduces more restrictions on the functional form. However, it generates more precise estimates as there are fewer parameters to be estimated. For the main results, we present both specifications side by side. They yield similar results. For the analysis of additional outcomes, we therefore mainly use the polynomial specification (2.2) as our preferred specification. We estimate all models using OLS and cluster standard errors at the state-cohort level. We also weight observations to account for different sample sizes in different years of observation.

Our key identifying assumption is that the immigration of family migrants is independent of state-level unemployment rates in the US in the year of immigration. As we have argued above, family migrants cannot choose their date of immigration based on economic conditions. The combination of long waiting times, uncertainty about the length of waiting times, and the short validity of the visa does not allow them to synchronize their arrival with labor market condition in the US. In addition, family migrants typically join their sponsor's household in the US. Own calculations using administrative data on the universe of family migrants from the Philippines to the US indicate that this is the case for 98.5 percent of all family migrants. Hence, family migrants do not choose the US state based on economic conditions either. We therefore argue that the local economic conditions family migrants face at arrival in the US are exogenous.

Our data allows us to conduct an indirect test of this identifying assumption. If family-based migration is indeed exogenous to local economic conditions, state-level unemployment rates in the year of immigration should not be correlated with the size and composition of family migrants in US states. We test this prediction by regressing the state-level unemployment rate in the year of arrival on (i) individual-level migrant characteristics and (ii) aggregates of the size and composition of family migrants at the state-year-of-immigration level. Table 2.2 shows the results.

In the left panel, we restrict the sample to migrants who were interviewed in the first two years of arrival, thus minimizing the potential of subsequent internal or return migration. In the right panel, we use our full sample. All regressions include state and year of immigration fixed effects. For the sample of recent arrivals, none of the

**Table 2.2:** Balancing of characteristics by state unemployment rate at arrival

	First two years		Full sample	
	Individual	Aggregated	Individual	Aggregated
Age at immigration	-0.000 (0.001)	0.002 (0.005)	0.000 (0.000)	0.000 (0.003)
Female (0/1)	-0.011 (0.013)	0.065 (0.124)	0.018*** (0.006)	0.098* (0.056)
Years of schooling	-0.000 (0.002)	0.006 (0.014)	-0.003*** (0.001)	-0.008 (0.005)
Number of migrants (log)		-0.067 (0.053)		-0.064*** (0.024)
<i>N</i>	16024	849	88357	4143
F-Test	0.260	0.198	7.574	1.803

Notes: Coefficients are from regressions of the state-level unemployment rates in the year of immigration on the respective variables. The sample for columns (1) and (2) is restricted to family migrants who immigrated in the two years prior to observation. The sample for columns (3) and (4) consists of all family migrants. Columns (1) and (3) use individual level data. Columns (2) and (4) use data aggregated on the state-cohort level. All regressions includes state and year-of-immigration fixed effects. In the regressions using individual data also birthplace fixed effects are included. Standard errors are clustered at the state-cohort level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

coefficients is statistically significant. The F-statistic from a test of joint significance comes to the same conclusion. For the full sample, gender and years of schooling turn out to be statistically significant. However, they are far from economically significant. The coefficients suggest that being female is associated with only a 0.018 pp higher unemployment rate and an additional year of schooling with only a 0.003 pp lower unemployment rate. The strength of these associations is negligible. Overall, the results in Table 2.2 support our argument that migration decisions of family migrants are not endogenous to economic conditions.

Measuring the unemployment rate at the state rather than the national level provides us with a more accurate measure of economic conditions at arrival. Figure B.2 shows that there is considerable heterogeneity in unemployment rates between states over time, even after accounting for state and year fixed effects.

In our data, we only observe the current state of residence and the state of residence in the previous year. We thus need to proxy for the state of arrival using the state of residence in the previous year. To the extent that migrants move between states, however, we measure initial unemployment rates with an error. Figure B.3 shows that family migrants are less likely to move between states than employment-based migrants. This is likely due to strong family ties. Still, in the first five years after arrival, more than three percent of family migrants report having moved between states in the prior year. However, as we show later, family migrants do not seem to

move in response to initial economic conditions. If interstate migration is independent of initial economic conditions, measurement error will increase over time. We would then overestimate the speed of convergence because of potential attenuation bias. Our results are therefore conservative.

Given the cross-sectional nature of our data, we are not able to track individuals over time. Selective return migration could thus potentially bias our results (Dustmann and Görlach, 2015). For instance, we may wrongly conclude that immigrating into a recession is not associated with poor labor market outcomes if those immigrants with poor labor market outcomes decide to leave the US and disappear from our sample.

To test for return migration, Figure B.4 plots the number of migrants observed in year  $t$  relative to the number of migrants in the year of immigration  $m$  for different years of immigration over time. In the case of family migrants, there is no evidence for return migration. If anything, the number of family migrants observed in the sample increases over time.<sup>7</sup> Previous research documenting that family migrants, and those with Asian background in particular, are least likely to return (Dustmann and Görlach, 2015) and Borjas and Bratsberg (1996) is in line with this conclusion. In the case of employment-based migrants, the picture looks different. Their number in the sample substantially decreases over time, suggesting that a high share of migrants return. This result is consistent with recent evidence from Akee and Jones (2019). They use panel data and show that almost 40 percent of migrants to the US have returned within ten years after arrival.

We also conduct a more formal econometric test that combines the information from different years of immigration. We first aggregate the data at the year-of-immigration and years-in-the US level using person weights. We then regress the log number of family migrants on a full set of year-of-immigration and years-in-the-US dummies. We restrict the sample to individuals younger or equal to 50 years at the time of immigration to avoid mechanical censoring at the age of 60. The coefficients of the years-in-the-US dummies indicate the number of migrants observed in year  $t$  relative to the first year. In the absence of return migration, the coefficients should be close to zero and stable over time. For family migrants, the coefficients are indeed close to zero, and there is no trend over time. For employment-based migrants, however, the coefficients become increasingly negative, suggesting substantial return migration. The econometric evidence on return migration is hence in line with the descriptive evidence presented above.

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<sup>7</sup> Figure B.5 displays the change in cohort size over time aggregated for all years of immigration for family migrants, Filipinos and employment migrants. It confirms that there is no change in cohort size for family migrants and Filipinos. However, the cohort size of employment migrants decreases significantly over time.

## 2.6 Results

### 2.6.1 The Long-term Effects of Initial Conditions on Labor Market Outcomes of Family Migrants

Our main focus is on how unemployment rates at arrival affect the economic integration of family migrants over the first ten years in the US. Figure 2.3 shows the effect of initial unemployment rates on four key labor market outcomes: employment, log real annual wage income, log real hourly wages, and occupational quality as measured by the occupational income score. The figure shows how much a one pp increase in the initial unemployment rate affects each outcome. It plots the year-specific coefficient  $\beta$  for the first ten years after arrival. Red dots refer to the flexible specification (Equation 2.1), blue dots including the 95 percent confidence interval to the more restrictive polynomial specification (Equation 2.2). The results for both specifications are very similar. In the following, we therefore only describe the results from the polynomial specification. The corresponding regression tables are in Table B.3 in the appendix.

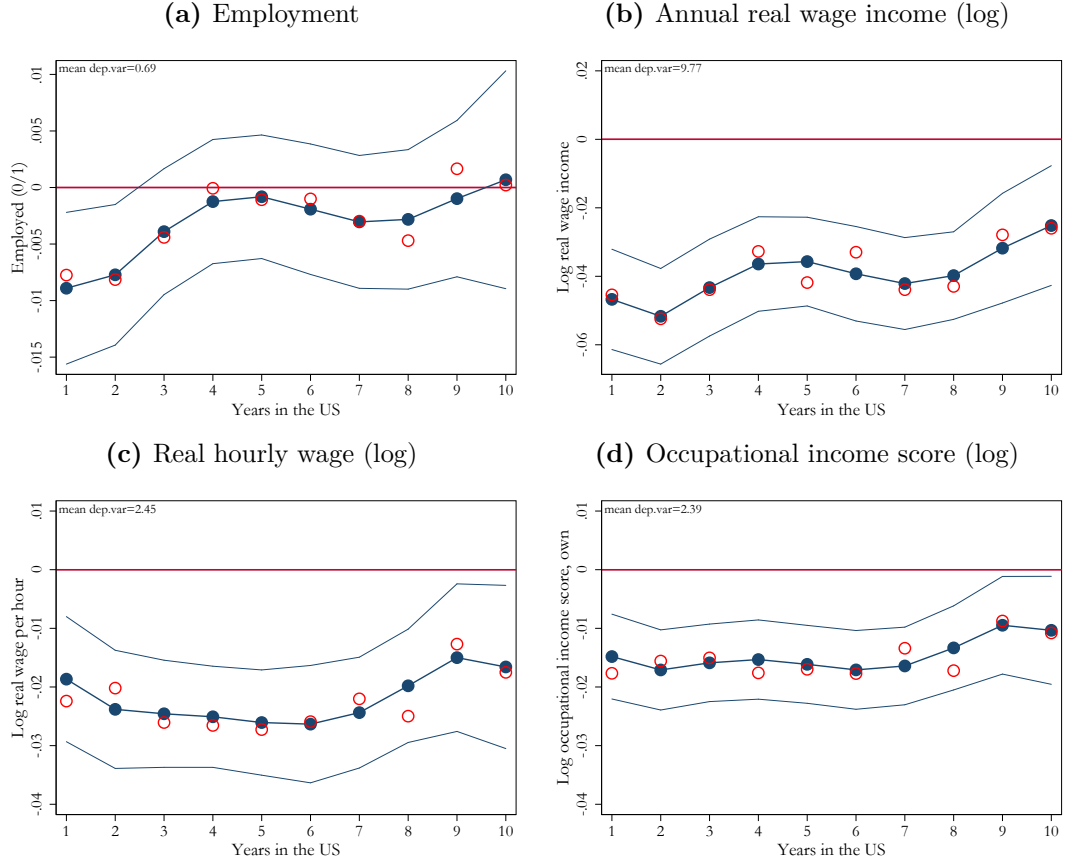
A one pp increase in the initial unemployment rate has only a small and short-lasting effect on the employment status of family migrants (Panel (a) of Figure 2.3). In the first year after arrival, family migrants are 0.9 pp less likely to be employed. Compared to a scenario where family migrants have the same labor force participation rate (LFPR) and propensity to be employed as the overall working-age population, this effect is relatively small. In that case, a one pp increase in the initial unemployment rate should lower the initial employment rate by  $\frac{1}{LFPR}$ . With an LFPR of about 0.75, this effect amounts to about 1.3 pp. Family migrants hence do relatively well in terms of finding a job. Consistent with this result, employment rates converge quickly and do not differ by initial unemployment rates after four years in the US.

The picture looks different for annual wage income (Panel (b) of Figure 2.3). Conditional on being employed, a one pp increase in the initial unemployment rate decreases annual wage income in the first four years by about four percent. There is only slow convergence afterward to a persistent and statistically significant negative effect of about 2.5 percent.

The negative effect on wage income is largely due to lower wage rates (Panel(c) of Figure 2.3). A one pp increase in the initial unemployment rate decreases hourly wages by about two percent. The effect is persistent and relatively constant over time, suggesting that family migrants who arrive at times of high unemployment permanently face lower wage rates. The gap between the effect on annual wage income and the effect on hourly wages indicates that family migrants who immigrate into a higher unemployment work fewer hours.



**Figure 2.3:** Persistent effects of unemployment rate at immigration on labor market outcomes for family migrants



Notes: The analysis is based on data from the 2000 US Census and the 2001 to 2017 American Community Survey. The sample is restricted to individuals who were born outside the US as non-US citizens and immigrated between 1990 and 2016. We further restrict the sample to individuals 22 to 60 years old at the time of immigration and of observation. To identify family migrants, we restrict to the countries of origin with mostly family migration as outlined in Section 2.4. This leaves us with a sample of 88,357 migrants overall and out of which 65,565 are employed. Observations are weighted by year, i.e., the mean of person weights in the respective observation year. The x-axis displays the years since migration. The y-axis shows the effect of a one pp higher state-level unemployment rate at immigration on the outcome variable in the respective year. The graph shows the result of two different specifications. The red hollow circles show the effect using the flexible specification from Equation 2.1. The connected blue dots show the marginal effects using the polynomial specification from Equation 2.2. Both specifications include dummies for years in the US, year of immigration, year of observation, and state of residence. They also include age, age-squared, gender, and years of schooling. The thin lines show the 95% confidence interval for the polynomial specification. Standard errors are clustered at the state-year-of-immigration-level.

There is also evidence for occupational downgrading (Panel (d) of Figure 2.3). A one pp increase in the initial unemployment rate decreases the occupational income score, which measures the log median wage income of a worker in the same occupation, by about 1.5 percent. Family migrants are hence pushed into lower-paid occupations. The effect is long-lasting. There is only slow convergence towards the end of the ten-year period under consideration.

To better understand the composition of the negative effect on wage income, Figure 2.4 decomposes the effect into three components: occupational downgrading as measured by the occupational income score, reduction in residual hourly wages (i.e., beyond the income loss due to occupational downgrading), and reduction in working hours.

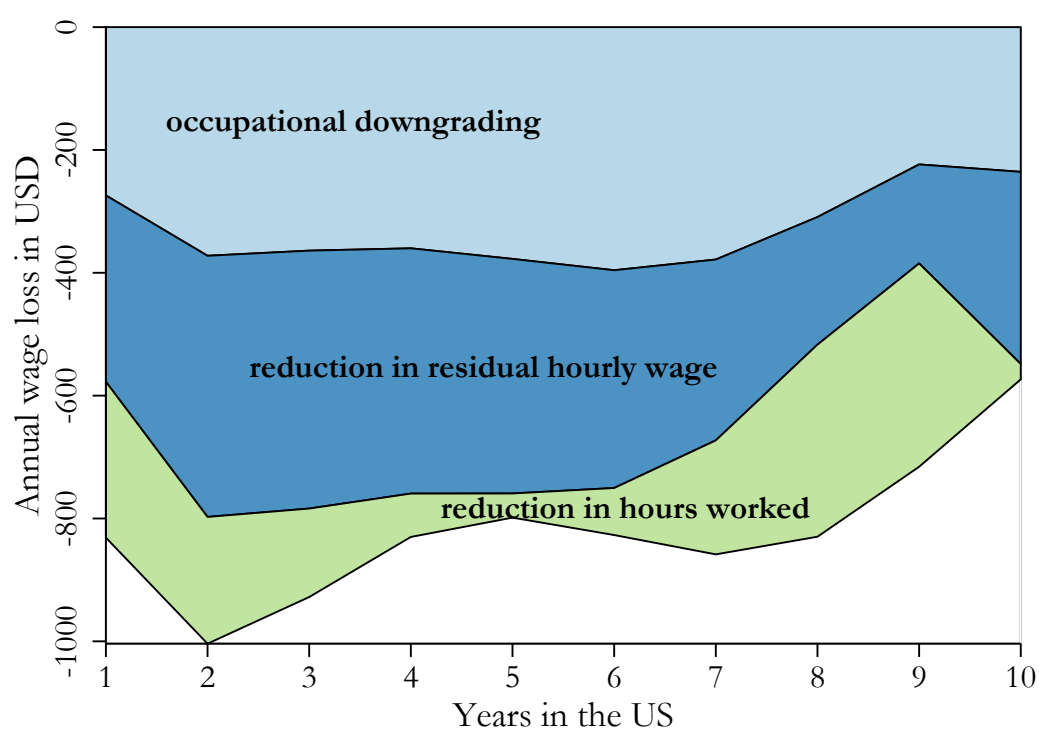
To do so, we first estimate the effect of a one pp increase in the initial unemployment rate on annual wage income (a). We then calculate the average hours worked for every year since immigration. We obtain the reduction in annual wage income due to a reduction in the wage rate by multiplying the effect on annual wage income per hour times the average annual hours worked (b). Subtracting (b) from (a) gives us the reduction in wage income due to a reduction in hours worked. We further decompose (b) into an effect driven by occupational downgrading (c) and an effect on the hourly wage rate holding constant the occupation (residual hourly wage). We estimate the reduction due to occupational downgrading by multiplying the effect on occupational hourly wages times the average annual hours worked. The difference between (c) and (b) gives us the effect on residual hourly wages.

All three components play a considerable role, but the reduction in working hours is quantitatively less important than occupational downgrading and the reduction in residual hourly wages. Overall, a one pp increase in the initial unemployment rate decreases annual wage income by USD 800-1,000 in the first years and by USD 600 after ten years. The estimated cumulative loss in wage income after ten years amounts to about USD 8,200. A recession, which roughly corresponds to a three pp increase in unemployment rates, would hence reduce the ten-year income of the average family migrant by USD 25,000.

For comparison, Figure 2.5 shows the main results for the subgroup of Filipino migrants. The results are similar. There are only small effects on employment, and the effect on wage income is also slightly lower for Filipino migrants.

We also compare the effects to college graduates, who might serve as a natural benchmark. To do so, we replicate the results for college graduates using the ACS data and our econometric specification. We restrict the sample to US-born college graduates and replace the year of immigration with the year of graduation. As Figure 2.5 shows,

**Figure 2.4:** Decomposition of the effect of the unemployment rate at immigration on annual wage income



Notes: The figure shows the absolute loss in annual wage income as a result of a one pp increase in unemployment rates. The light blue area depicts the loss due to occupational downgrading, the dark blue area the loss due to a reduction in residual hourly wage (i.e., beyond the wage reduction due to occupational downgrading), and the green area the loss due to a reduction in working hours.

the results obtained using our specification are similar to the results in Kahn (2010), Oreopoulos et al. (2012), and Altonji et al. (2016). While there are no differences in the effect on employment between family migrants and college graduates, the effect on wage income is substantially smaller for college graduates and disappears ten years after graduation. College graduates are hence more able to cope with adverse initial conditions than family migrants.

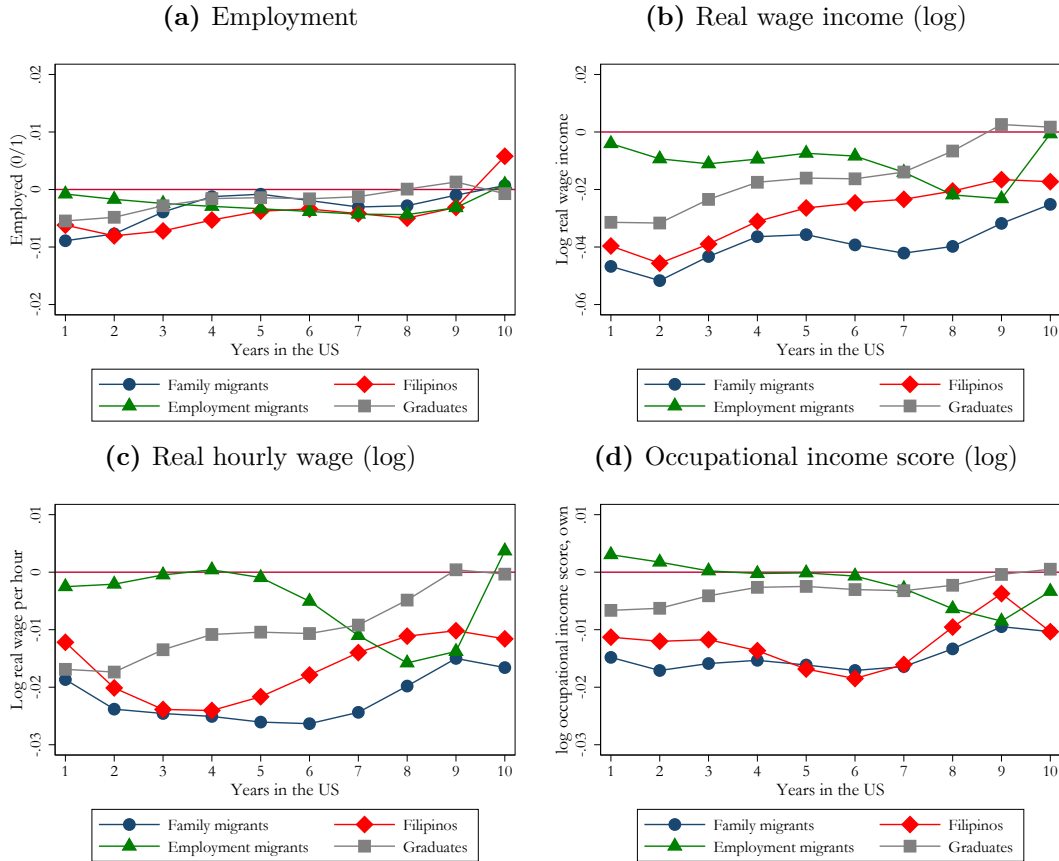
We extend our main results and test for effect heterogeneity by gender and level of education. There are no systematic differences for male than female family migrants (Figure B.6). Also the effect for different levels of education look similar (Figure B.7). If anything, we see more convergence for high educated family migrants in terms of hourly wages and wage income. Only for occupational downgrading high educated family migrants seem to respond a bit stronger, which could reflect that the potential for occupational downgrading is larger for high-skilled than low-skilled individuals.<sup>8</sup>

Finally, we check the robustness of our main results to using alternative econometric specifications. First, we account for the possibility that economic conditions at the time of observation also affects labor market outcomes. To the extent that the current and initial economic conditions are correlated, our results reflect the effect of both initial and subsequent conditions. We address this issue by using state-year-of-observation fixed effects. Our results are almost unchanged (Figure B.9 in the appendix). Second, we allow for a more comprehensive definition of initial conditions and define the treatment as the average state-level unemployment rate in the first three years after arrival. Doing so, we might better capture whether individuals immigrate into a recession or a boom. Our results are similar. The effects become even stronger (Figure B.10 in the appendix). Third, we go beyond wage income and also investigate the effects on self-employment, labor income (i.e., wage, business, and farm income), and total earnings (i.e., labor income and all other forms of income including transfers). Our results are robust to these more comprehensive definitions of income (Figure B.11 in the appendix). There is only a small and statistically insignificant effect on self-employment.

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<sup>8</sup> The level of education is potentially endogenous. Migrants may choose to invest in additional human capital in response to initial economic conditions. To minimize this possibility, we re-run the heterogeneity analysis restricting the sample to migrants aged 30 and above. For those migrants, the level of education should be more exogenous. Our results hold (see Figure B.8 in the appendix).

**Figure 2.5:** Comparison of the effects of the unemployment rate at immigration for family migrants, employment migrants and college graduates



Notes: The analysis is based on data from the 2000 US Census and the 2001 to 2017 American Community Survey. The figure depicts the analysis for four distinct samples: Family migrants (blue dots), Filipinos (red diamonds), employment migrants (green triangles), and college graduates (grey squares). Section 2.4 outlines the construction of these samples. The immigrant samples are restricted to individuals who were born outside the US as non-US citizens and immigrated between 1990 and 2016. The graduation sample is restricted to US college graduates who graduated between 1990 and 2016. We further restrict the sample to individuals 22 to 60 years old at the time of immigration/graduation and of observation. Observations are weighted by year, i.e., the mean of person weights in the respective observation year. The x-axis displays the years since migration/graduation. The y-axis shows the effect of a one pp higher state-level unemployment rate at immigration on the outcome variable in the respective year. The graph shows the marginal effects using the polynomial specification from Equation 2.2 for the different samples. All specifications include dummies for years in the US/since graduation, year of immigration/graduation, year of observation, and state of residence. They also include age, age-squared, gender, and years of schooling.

### 2.6.2 Coping Strategies

#### Ethnic Networks

A large literature has shown that ethnic (migrant) networks substantially help immigrants to find a job (e.g., Munshi, 2003 or Patel and Vella, 2013). The support provided by ethnic networks might be particularly beneficial in a recession. The scarcity of jobs will make it more difficult for recent immigrants to compete in the labor market since they have low levels of destination-specific human capital as well as little destination-specific experience that can signal their ability.

To test this prediction, we first investigate whether the employment and wage effects of immigrating into a recession differ by the size of the ethnic network. We do so by adding interactions between initial conditions and network size to our previous specification. We define network size as the population share of co-ethnics living in the same state. We calculate this variable for three points in time: for 1990 and 2000 based on census data and for 2010 based on the aggregate data from the ACS waves conducted in 2009, 2010, and 2011. The initial network size of each migrant in our sample is then given by the value of the network variable in the year that predates the year of immigration. For instance, a migrant who arrived in 2003 is assigned the network as measured in 2000. We conduct this analysis only for Filipinos, who by far constitute the largest group of family migrants. The samples are too small for other countries with predominantly family-based migration, especially for the analysis of ethnic occupations as introduced below. The following results thus focus on Filipinos. We use terciles to distinguish between small, medium, and large networks and estimate the following equation:

$$\begin{aligned}
 y_{i,s,t,m} = & \alpha + \beta_1 UR_{s,m} + \beta_2 UR_{s,m} \times Net_{terc2} + \beta_3 UR_{s,m} \times Net_{terc3} \\
 & + \delta_1 Net_{terc2} + \delta_3 Net_{terc3} + \gamma_{(t-m)} + \theta_s + \lambda_t + \chi_m + \mathbf{X}_{i,t}\Delta + \epsilon_{i,t}
 \end{aligned} \tag{2.3}$$

To capture the initial effects, we restrict the sample to migrants who have not spent more than five years in the US. We also look at average effects over this time period to increase the sample size. Table 2.3 shows the results. In states with small networks (first tercile), a one pp higher initial unemployment rate lowers employment by 2.1 pp in the first five years. The effect is about 75 percent smaller (-0.5 pp) in areas with medium or large networks. Similarly, the effect on wage income is largest in areas with small networks (-5.2%) and smaller in areas with medium or large networks. However, the interactions terms are not or only marginally significant. These results suggest that ethnic networks can mitigate the negative labor market effects of adverse economic conditions at arrival. They seem to be particularly helpful for finding

employment.

We also investigate whether adverse initial conditions push migrants to enter occupations with large co-ethnic networks. We follow Patel and Vella (2013) and construct a measure of the concentration of co-ethnics in different occupations:

$$concentration_{e,o,t} = \frac{100 * mig_{e,o,t}}{mig_{e,t}}$$

where  $concentration_{e,o,t}$  measures the percent of workers from ethnicity  $e$  in year  $t$  that are employed in occupation  $o$ . High values of  $network_{e,o,t}$  indicate that workers from a given ethnicity concentrate in these occupations. We distinguish between 86 occupations and calculate the measure again for the years 1990, 2000, and 2010.

Column (3) of Table 2.3 shows that adverse initial conditions do not cause family migrants to enter occupations with high concentrations of co-ethnics in areas with small networks. In areas with medium or large networks, however, family migrants are considerably more likely to enter such occupations. As a comparison, we conduct the same analysis with migrants from India, the largest origin country classified as predominantly employment-based (Table B.5). For these migrants we do not expect networks to matter in the same way since they usually have a job already before arriving. Furthermore, we would expect selection issues to be prevalent as their immigration might be tied to economic conditions. Indeed, patterns are very different for employment migrants.

We also run a placebo regression and check whether family migrants are more likely to enter occupations in which many US-born workers, i.e, generally large occupation, or other migrant groups are employed. For this exercise, we calculate the concentration measure for US-born workers and for the two most important countries of origin, Mexico and China, and use them as outcomes. The last three columns in Table 2.3 summarize the results. Higher initial unemployment rates do not make family migrants more likely to work in occupations with higher concentrations of natives, Mexicans, or Chinese. The point estimates are close to zero, and only one of the nine coefficients is marginally significant.

Taken together, these results suggest that ethnic networks play a significant role in mitigating the negative consequences of immigrating into a recession for family migrants. Ethnic networks facilitate the job search of arriving family migrants and thus increase their likelihood to work in occupations with strong ethnic networks.

**Table 2.3:** Effect of a one pp increase in initial unemployment rates by network strength

	Effect of initial conditions			Placebo		
	Employ- ment	Wage income	% Filipinos in occ.	% natives in occ.	% Mexicans in occ.	% Chinese in occ.
UR at immigration	-0.021*** (0.006)	-0.052*** (0.015)	-0.044 (0.044)	-0.015 (0.024)	0.024 (0.044)	0.042 (0.044)
× medium network	0.016*** (0.004)	0.016 (0.010)	0.137*** (0.030)	0.030* (0.017)	-0.000 (0.031)	0.025 (0.027)
× large network	0.014** (0.006)	0.029* (0.016)	0.279*** (0.050)	0.008 (0.025)	0.002 (0.045)	-0.000 (0.045)
Observations	23967	17896	17896	17896	17896	17896

Notes: The analysis is based on data from the 2000 US Census and the 2001 to 2017 American Community Survey. The analysis is restricted to Filipinos that resided in the US for at most five years. See Section 2.4 for further details on the sample construction. Observations are weighted by year, i.e., the mean of person weights in the respective observation year. All specifications include dummies for years in the US, year of immigration, year of observation, and state of residence. They also include age, age-squared, gender, and years of schooling. The outcome variables in columns four to six are the share of workers in the respective group that works in the occupation (in percent). The variable is calculated at the national level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

### Family Support and Welfare Benefits

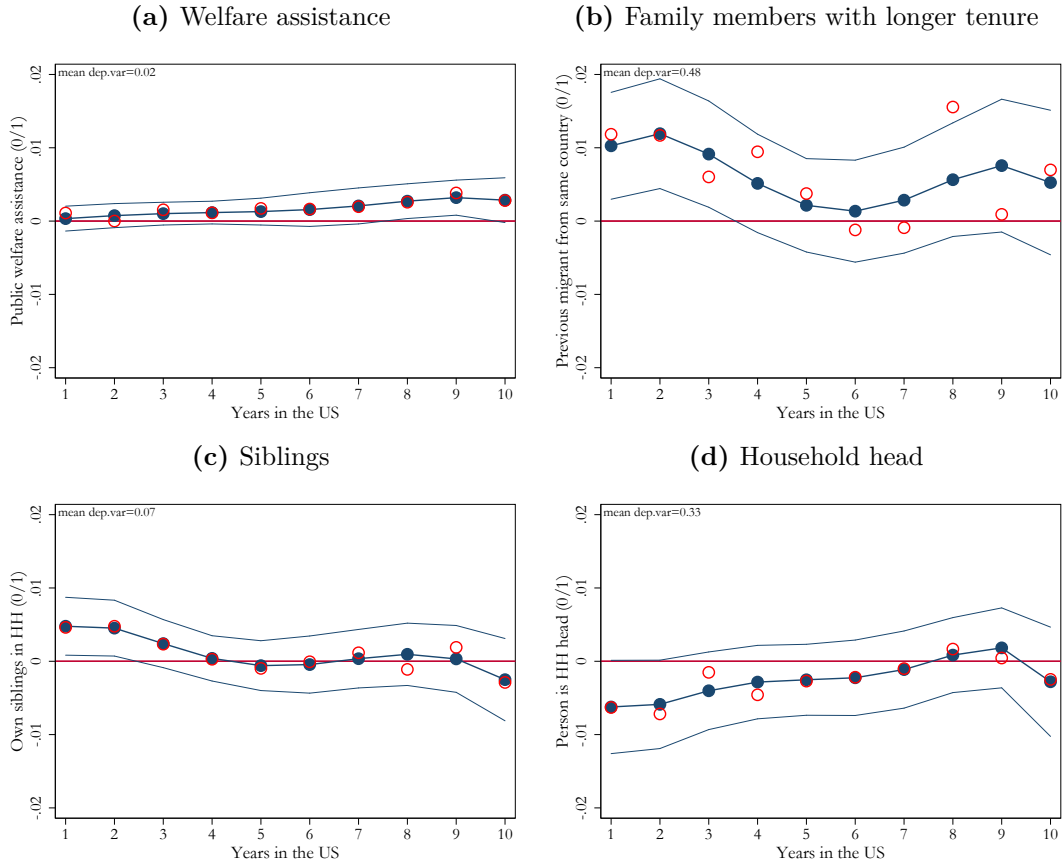
Family migrants are generally not entitled to welfare benefits for at least five years after arrival. Sponsors have to sign an affidavit of financial support, which legally obliges them to support sponsored family members for ten years or until they become a US citizen. The ACS provides information on welfare receipt and the composition of household members, which we can use as a proxy for receiving support from the family.

Panel (a) of Figure 2.6 confirms that adverse economic conditions at arrival have no effect on receiving welfare assistance. The remaining panels of Figure 2.6 show that family migrants rely on support from the family instead. A one pp increase in the initial unemployment rate increases the likelihood of living with a sibling by 0.5 pp and the likelihood of living with a family member with longer tenure in the US by 1 pp. It also decreases the likelihood of being a household head by 0.5 pp. These effects largely disappear over time. But they suggest that adverse economic conditions make it more difficult for recently arrived family migrants to move out of their sponsors' households because they depend on support from the family. This interpretation could also explain why initial unemployment rates have only a small effect on employment, but relatively large effects on wage income and occupational quality. Family migrants might try to minimize the burden they place on their sponsors by accepting jobs below their qualifications.



The immobile support received from the family could also reduce the geographic mobility of family migrants. In general, the literature has found that migrants are more mobile than natives (Green, 1999, Braun and Kvasnicka, 2014, and Cadena and Kovak, 2016). Figure B.12, however, shows that family migrants do not increase their geographical mobility in response to local economic conditions at arrival. If anything, they are less likely to move. The point estimates, however, are small and not statistically significant. Family migrants thus do not seem to contribute to 'greasing the wheels of the labor market' (Borjas, 2001). By contrast, their increased dependence on family members likely increases their job search frictions.

**Figure 2.6:** Effects of the unemployment rate at immigration on receiving welfare assistance and support from the family



Notes: The analysis is based on data from the 2000 US Census and the 2001 to 2017 American Community Survey. The sample is restricted to individuals who were born outside the US as non-US citizens and immigrated between 1990 and 2016. We further restrict the sample to individuals 22 to 60 years old at the time of immigration and of observation. To identify family migrants, we restrict to the countries of origin with mostly family migration as outlined in Section 2.4. This leaves us with a sample of 88,357 migrants. Observations are weighted by year, i.e., the mean of person weights in the respective observation year. The x-axis displays the years since migration. The y-axis shows the effect of a one pp higher state-level unemployment rate at immigration on the outcome variable in the respective year. The graph shows the result of two different specifications. The red hollow circles show the effect using the flexible specification from Equation 2.1. The connected blue dots show the marginal effects using the polynomial specification from Equation 2.2. Both specifications include dummies for years in the US, year of immigration, year of observation, and state of residence. They also include age, age-squared, gender, and years of schooling. The thin lines show the 95% confidence interval for the polynomial specification. Standard errors are clustered at the state-year-of-immigration-level.

### 2.6.3 Alternative Strategies for Identifying Family Migrants

Our identifying strategy builds on the idea that, due to long waiting times for a visa, family migrants cannot choose their date of immigration based on economic conditions. As a result, the local economic conditions family migrants encounter upon arrival in the US are exogenous. Currently available datasets, however, do not allow us to identify family migrants precisely. We have therefore restricted the sample to immigrants from countries for which family-based migration is the dominant mode of migration to the US. Yet, the sample still includes many employment-based migrants and immediate relatives who do not face long waiting times (see Table B.1 in the appendix). Their migration decisions may well be endogenous to economic conditions.

In case of endogenous migration decisions, however, our estimates are likely to be downward-biased. By definition, employment-based migrants arrive with a job. Their economic integration should be less susceptible to labor market conditions at the time of arrival. To test this prediction, we restrict the sample to immigrants from countries with predominantly employment-based migration and re-run the main analysis. Figure 2.5 shows that initial unemployment rates have indeed only a small effect on employment-based migrants. The effects are much smaller than for family migrants. These results provide a useful benchmark for potential biases. They demonstrate that the effects for family migrants are potentially larger than our main results suggest.

When it comes to immediate relatives, sponsors might be more likely to select more productive family members in times of high unemployment. Otherwise, they might find themselves obliged to offer financial support. A positive selection of immediate relatives would again bias our estimates downwards. Unfortunately, our data does not allow us to test this argument. In any case, as we have shown in Table 2.2, initial unemployment rates are not correlated with observable migrant characteristics such as age, education, and gender. The potential for donors to endogenously sponsor family members might thus be limited.

We also offer two alternative strategies for identifying family migrants with waiting times. In our first strategy, we use data from the Philippine government that provide information on the visa type for permanent migrants from the Philippines to the US since 1988. This administrative data capture the universe of migrants as every permanent migrant from the Philippines needs to register with the Commission on Filipinos (CFO) overseas for departure clearance. In addition to the exact admission category, the data provides migrant-level information on demographic characteristics, date of migration, and the destination state in the US. We use the information on admission category, age, gender, education, and destination state to reweigh our sample of Filipino migrants in the ACS, so they resemble the characteristics of family migrants

with waiting times. Compared to Filipino migrants in our sample, the age at immigration of family migrants with waiting times in the CFO data is on average higher (39 vs. 35 years), and CFO family migrants are less likely to be female (53% vs. 65%) and less likely to have a college degree (55% vs. 61%). Nevertheless, our results for the subsample of Filipino migrants hold. As Figure 2.7 shows, the reweighed results are similar to the previous results without such weights.

In our second strategy, we try to identify immigrant spouses with waiting times based on the timing of immigration (similar to the method used by Borjas and Bronars, 1991). We focus on spouses, not other relatives, as they have the highest likelihood of living in the same household as their sponsor. We can thus capture them in our data. Spouses of LPRs waited between three to five years for their F2A visas over most of the period under consideration (Figure 2.1). We identify spouses in the ACS data by restricting the sample to individuals who live in the same household as their spouse and sponsor and whose spouse immigrated at least as many years ago as the relevant waiting period before them and at most ten years later than that.<sup>9</sup>

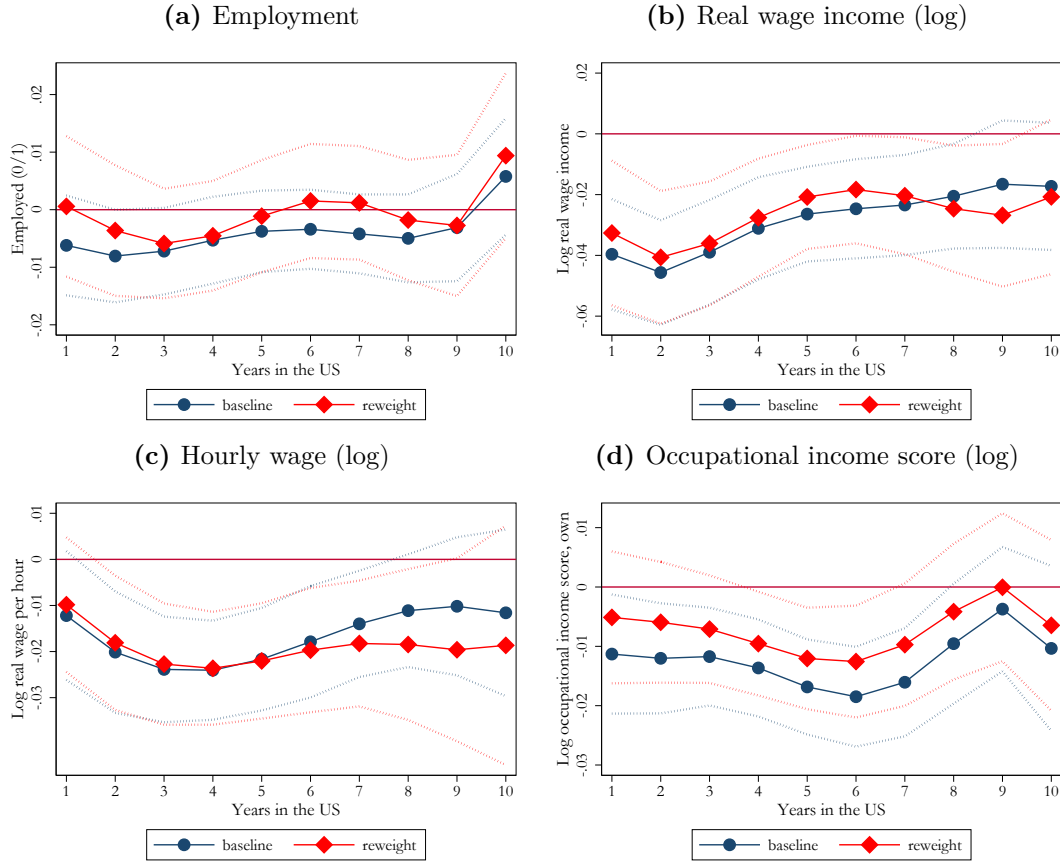
As before, the sample only includes individuals who were between 22 and 60 years old at the time of immigration and the time of observation. This time, however, we consider immigrants from all countries of origin except Mexico and Central American countries, where irregular migration plays a large role. We thus have a more even distribution of countries of origin. The most important countries of origin of LPR spouses are India (16%), the Philippines (8%), China (8%), and Vietnam (5%).

Figure 2.8 shows our results for the sample of LPR spouses. They are in line with our main results. Only the effect on the occupational income score is less pronounced for LPR spouses. We also test our ability to correctly capture LPR spouses by regressing the difference in the year of immigration between the two spouses on official waiting times for different types of family visa. Reassuringly, the coefficient for the F2A waiting time is 0.93. Coefficients for all other types of family visa are substantially lower (see Table B.4 in the appendix). Overall, the above evidence suggests that our results are robust to alternative approaches to identifying family migrants.

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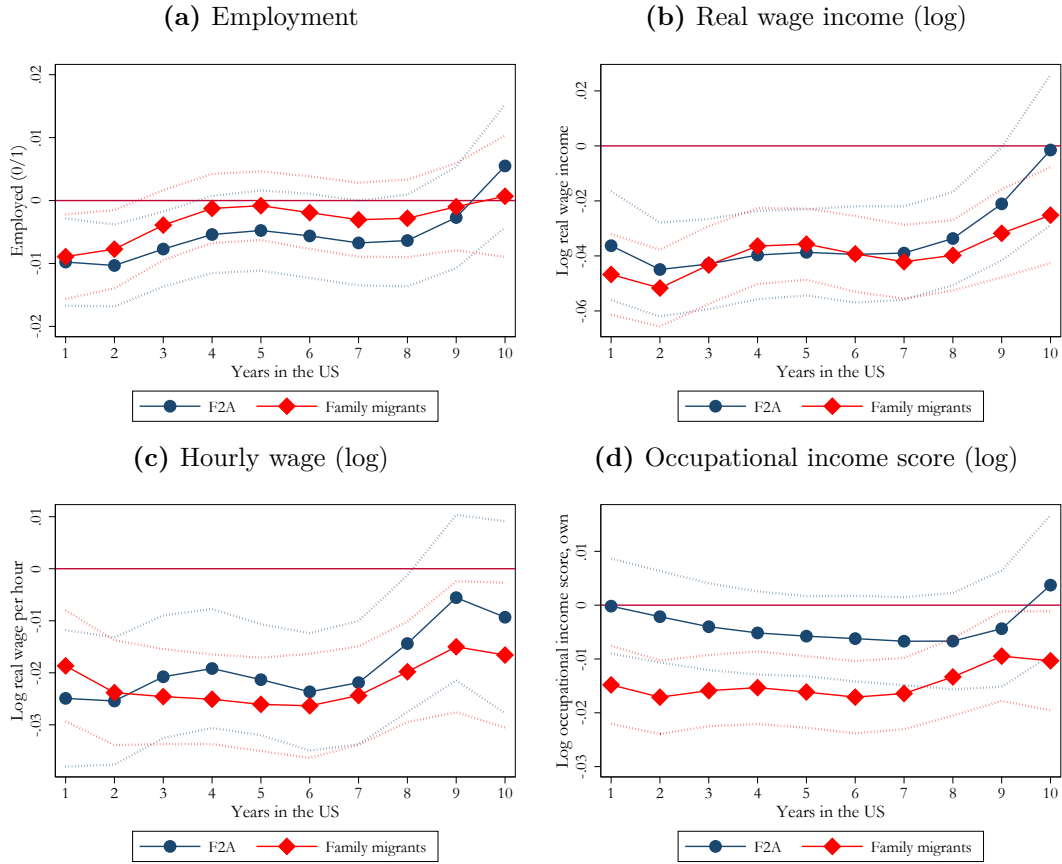
<sup>9</sup> Information on the year of marriage and on citizenship is only available from 2008 onwards. We are thus not able to exploit this additional information for our analysis.

**Figure 2.7:** Labor market effects of the unemployment rate at immigration for immigrants from the Philippines with and without weights



Notes: The analysis is based on data from the 2000 US Census and the 2001 to 2017 American Community Survey. The sample is restricted to individuals who were born in the Philippines as non-US citizens and immigrated between 1990 and 2016. We further restrict the sample to individuals 22 to 60 years old at the time of immigration and of observation. The graph shows the marginal effects using the polynomial specification from Equation 2.2 using different weights. The blue dots depict coefficients using year weights, i.e., the mean of person weights in the respective observation year. The red diamonds display coefficients using weights based on administrative Filipino emigrant data obtained from CFO. In particular, information on admission category, age, gender, education, and destination state is used to reweigh our sample of Filipino migrants in the ACS, so they resemble the characteristics of family migrants with waiting times. The x-axis displays the years since migration. The y-axis shows the effect of a one pp higher state-level unemployment rate at immigration on the outcome variable in the respective year. Both specifications include dummies for years in the US, year of immigration, year of observation, and state of residence. They also include age, age-squared, gender, and years of schooling.

**Figure 2.8:** Labor market effects of the unemployment rate at immigration for spouses of legal permanent residents (F2A)



Notes: The analysis is based on data from the 2000 US Census and the 2001 to 2017 American Community Survey. The figure depicts the analysis for two distinct samples: F2As (blue dots), and Family migrants (red diamonds). Section 2.4 outlines the construction of these samples. The samples are restricted to individuals who were born outside the US as non-US citizens and immigrated between 1990 and 2016. The F2A sample is restricted to spouses of prior migrants. We further restrict the sample to individuals 22 to 60 years old at the time of immigration and of observation. Observations are weighted by year, i.e., the mean of person weights in the respective observation year. The x-axis displays the years since migration. The y-axis shows the effect of a one pp higher state-level unemployment rate at immigration on the outcome variable in the respective year. The figure shows the results from estimating Equation 2.2 for the different samples. Both specifications include dummies for years in the US/since graduation, year of immigration/graduation, year of observation, and state of residence. They also include age, age-squared, gender, and years of schooling.

## 2.7 Conclusion

The economic integration of immigrants is key to reaping the benefits of international migration for immigrants and citizens of destination countries alike. Yet, despite considerable policy efforts, immigrant integration is often imperfect and varies substantially across and within migrant cohorts. A large literature has examined the economic assimilation of immigrants over time, paying particular attention to changes in immigrant characteristics and selective return migration.

In this paper, we take a different perspective and analyze how economic conditions at the time of arrival shape the economic integration of immigrants in the US. We introduce a new identification strategy that exploits the inability of family migrants to synchronize their arrival with labor market conditions in the US. Long waiting times resulting from caps on the yearly number of family-sponsored visa decouple the migration decision from economic conditions at the time of actual migration. As a consequence, some family migrants happen to immigrate into a recession, others into a boom. This strategy also allows us to provide the first evidence on the economic integration of family migrants, who constitute the largest group of permanent immigrants in the US and the OECD.

We find that a one pp higher unemployment rate at the time of arrival has little effect on employment, but decreases annual wage income by about four percent in the short run and by 2.5 percent in the longer run. This is the result of occupational downgrading. A potential explanation for the persistence of the effects may be the increase in search frictions as a result of receiving immobile support from the family. We document that immigrants who arrive at times of high unemployment are more likely to reside with family members, most likely the sponsor of their visa. They are hence less likely to engage in subsequent internal migration to better labor markets.

Our results show that the cap on the number of family-sponsored visas has important consequences for immigrant integration. They suggest that a more flexible cap that fluctuates pro-cyclically with economic conditions might considerably improve the economic integration of family migrants. Alternatively, allowing family-sponsored visa holders to delay their immigration by more than six months might also improve their ability to integrate into the US economy.

## Appendix B

**Table B.1:** Admission categories of migrants by country type

	Family countries Mean	Philippines Mean	Employment countries Mean	Other countries Mean
<b>Panel A: Year 2005</b>				
Family	67.0	52.0	4.19	23.0
<i>Immediate relatives</i>	43.5	33.5	3.81	18.0
<i>Family capped</i>	23.6	18.5	0.38	4.99
Employment	11.1	41.2	66.9	23.9
Refugee and diversity	6.17	0.11	0.35	13.9
Other	15.7	6.69	28.6	39.2
<b>Panel B: Year 2010</b>				
Family	71.5	60.8	3.53	24.4
<i>Immediate relatives</i>	46.6	39.8	3.20	19.4
<i>Family capped</i>	24.9	21.0	0.33	5.02
Employment	9.60	31.4	60.7	19.4
Refugee and diversity	4.93	0.081	0.38	13.2
Other	13.9	7.75	35.3	43.0
<b>Panel C: Year 2015</b>				
Family	68.1	53.7	2.79	23.3
<i>Immediate relatives</i>	44.0	35.2	2.56	18.0
<i>Family capped</i>	24.2	18.5	0.23	5.25
Employment	8.60	32.9	65.1	21.4
Refugee and diversity	3.34	0.033	0.39	12.7
Other	19.9	13.3	31.7	42.6

Notes: Data is obtained from the Yearbook of Immigration Statistics (US Department of Homeland Security). The yearbooks provide information on the number of immigrants and aliens who are admitted to the US by admission category, country of origin, and year. We only consider categories that are likely to be sampled by the census and the ACS. They include all different types of LPRs (family-based, employment-based, refugees and diversity migrants and others (including students and exchange visitors on temporary visas, and temporary workers). They exclude tourists, business travelers, and diplomats. For each country of origin, we then calculate the share of three different types of migrants distinguishing between family-based migrants, employment-based migrants, and all other migrants. We define the dominant mode to account for more than 50 percent of yearly admissions in each of the years 2005, 2010, and 2015. Doing so allows us to identify countries for which the dominant mode of migration is stable over time. We define countries as mixed when no mode of migration accounts for more than 50 percent in all three years. Countries with predominantly family-based migration include the Philippines, Vietnam, Cambodia, Laos, Yemen, Guyana, and some small island states. Countries with predominantly employment-based migration mostly consist of OECD countries. They also include Argentina, South Africa, and India. Other modes of migration only dominate in a few countries in sub-Saharan Africa. All other countries have mixed modes of migration to the US.



**Table B.2:** Summary statistics

	Family migrants	Filipinos	Employm. migrants	Natives	Graduates
	Mean/SD	Mean/SD	Mean/SD	Mean/SD	Mean/SD
<b>Personal characteristics</b>					
Female (0/1)	0.63 (0.48)	0.65 (0.48)	0.48 (0.50)	0.51 (0.50)	0.55 (0.50)
Age at immigration	35.1 (9.12)	35.0 (8.95)	31.4 (7.97)	.	.
Age	40.7 (9.25)	40.5 (9.01)	36.2 (8.26)	40.9 (11.2)	27.4 (2.84)
At least 4 years of college (0/1)	0.39 (0.49)	0.60 (0.49)	0.72 (0.45)	0.30 (0.46)	1 (0)
Years of schooling	12.9 (4.16)	14.7 (2.74)	15.8 (2.88)	13.6 (2.59)	16 (0)
<b>Economic conditions</b>					
Unemployment rate at arrival	6.14 (1.94)	6.15 (1.97)	5.88 (1.93)	.	5.90 (1.93)
<b>Labor market outcomes</b>					
Employed (0/1)	0.70 (0.46)	0.74 (0.44)	0.70 (0.46)	0.75 (0.43)	0.87 (0.34)
Real wage income in 1000 USD	25.2 (24.2)	28.9 (25.6)	53.1 (51.7)	34.9 (36.6)	31.1 (25.2)
Real wage per hour	14.9 (15.3)	16.8 (16.3)	26.6 (24.0)	18.1 (17.8)	16.5 (13.5)
Occupational income score	12.1 (6.46)	13.6 (6.86)	19.2 (8.20)	13.9 (6.13)	15.7 (5.80)
Self employed (0/1)	0.053 (0.22)	0.037 (0.19)	0.063 (0.24)	0.082 (0.28)	0.041 (0.20)
Real wage, business and farm income	25.1 (24.5)	28.8 (25.8)	52.7 (52.3)	35.2 (38.3)	31.2 (25.9)
Public welfare assistance (0/1)	0.017 (0.13)	0.0066 (0.081)	0.0029 (0.054)	0.017 (0.13)	0.0048 (0.069)
<b>Social outcomes</b>					
Own siblings in hh (0/1)	0.067 (0.25)	0.059 (0.24)	0.0062 (0.078)	0.046 (0.21)	0.086 (0.28)
Person is hh head (0/1)	0.32 (0.47)	0.33 (0.47)	0.48 (0.50)	0.53 (0.50)	0.47 (0.50)
Previous migrant in HH (0/1)	0.48 (0.50)	0.46 (0.50)	0.20 (0.40)	.	.
Moved within last year (0/1)	0.17 (0.37)	0.17 (0.38)	0.24 (0.43)	0.16 (0.37)	0.31 (0.46)
Moved b/w states last year (0/1)	0.024 (0.15)	0.027 (0.16)	0.063 (0.24)	0.027 (0.16)	0.071 (0.26)
Moved within state (0/1)	0.14 (0.35)	0.15 (0.35)	0.17 (0.38)	0.13 (0.34)	0.24 (0.43)
Observations	88,357	49,214	196,989	25,442,079	1,401,738

Notes: The analysis is based on data from the 2000 US Census and the 2001 to 2017 American Community Survey. The table depicts five distinct samples: Family migrants, Filipinos, employment migrants, natives, and college graduates. Section 2.4 outlines the construction of these samples. The immigrant samples are restricted to individuals who were born outside the US as non-US citizens and immigrated between 1990 and 2016. The graduation sample is restricted to US college graduates who graduated between 1990 and 2016. We further restrict the sample to individuals 22 to 60 years old at the time of immigration/graduation and of observation. Observations are weighted by year, i.e., the mean of person weights in the respective observation year.

**Table B.3:** Persistent effects of the unemployment rate at immigration on labor market outcomes for family migrants

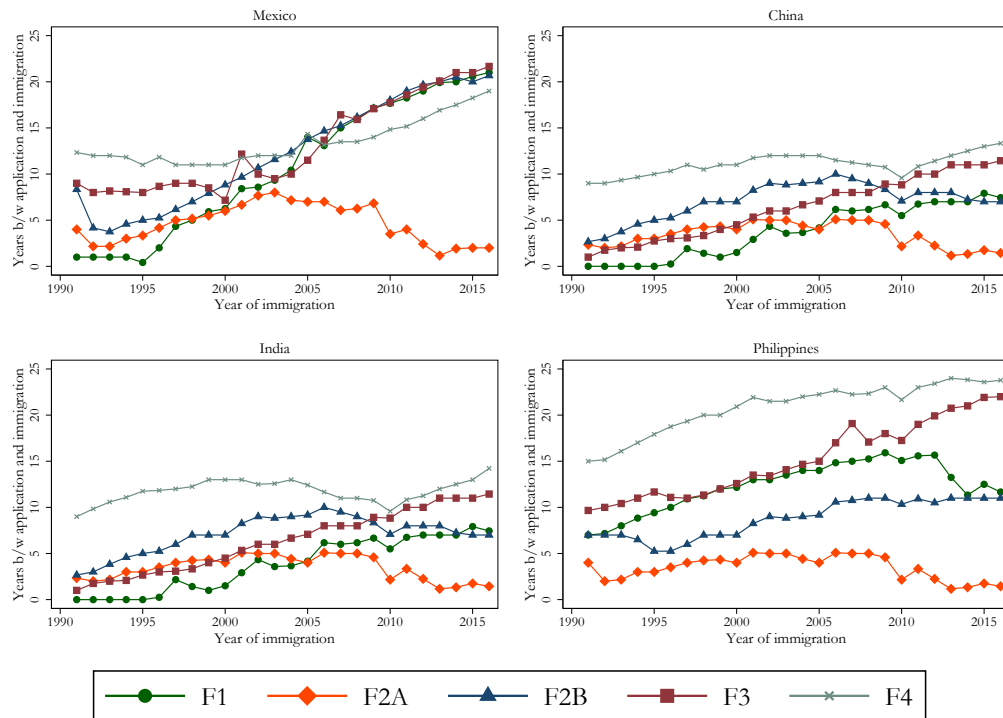
	Employment	Wage income	Hourly wage	Occscore
<b>Panel A: Effects by years in the US (x 100)</b>				
Year 1	-0.891*** (0.342)	-4.674*** (0.747)	-1.866*** (0.544)	-1.481*** (0.369)
Year 2	-0.772** (0.317)	-5.168*** (0.712)	-2.381*** (0.514)	-1.709*** (0.349)
Year 3	-0.390 (0.284)	-4.330*** (0.722)	-2.457*** (0.466)	-1.588*** (0.337)
Year 4	-0.125 (0.280)	-3.641*** (0.704)	-2.508*** (0.440)	-1.532*** (0.345)
Year 5	-0.082 (0.279)	-3.570*** (0.661)	-2.606*** (0.458)	-1.614*** (0.339)
Year 6	-0.192 (0.294)	-3.926*** (0.704)	-2.634*** (0.511)	-1.708*** (0.343)
Year 7	-0.304 (0.300)	-4.212*** (0.685)	-2.437*** (0.482)	-1.641*** (0.338)
Year 8	-0.282 (0.315)	-3.979*** (0.652)	-1.980*** (0.493)	-1.334*** (0.366)
Year 9	-0.098 (0.353)	-3.179*** (0.817)	-1.500** (0.642)	-0.947** (0.425)
Year 10	0.068 (0.491)	-2.517*** (0.892)	-1.659** (0.710)	-1.033** (0.470)
<b>Panel B: Polynomial coefficients (x 1000)</b>				
1st order	-17.552** (7.267)	-81.249*** (16.624)	-30.013** (11.739)	-25.602*** (7.938)
2nd order	10.861** (5.270)	42.870*** (12.928)	14.239 (8.838)	13.631** (5.888)
3rd order	-2.441* (1.403)	-9.192** (3.581)	-3.212 (2.458)	-3.154** (1.587)
4th order	0.233 (0.157)	0.864** (0.408)	0.335 (0.285)	0.325* (0.179)
5th order	-0.008 (0.006)	-0.029* (0.016)	-0.013 (0.012)	-0.012* (0.007)
Observations	88357	65565	65565	65509
Sample	MIG	MIG	MIG	MIG
Specification	Poly	Poly	Poly	Poly
State x Year FE	No	No	No	No

Notes: The analysis is based on data from the 2000 US Census and the 2001 to 2017 American Community Survey. The sample is restricted to individuals who were born outside the US as non-US citizens and immigrated between 1990 and 2016. We further restrict the sample to individuals 22 to 60 years old at the time of immigration and of observation. To identify family migrants, we restrict to the countries of origin with mostly family migration as outlined in Section 2.4. The sample size is 88,357 overall out of which 65,565 are employed. Observations are weighted the mean of person weights in the respective observation year. The table shows the effect of a one pp higher state-level unemployment rate at immigration on the outcome variable in the respective year. Panel A shows the effect using the flexible specification from Equation (2.1). Panel B shows the marginal effects using the polynomial specification from Equation (2.2). Both specifications include dummies for years in the US, year of immigration, year of observation and state of residence. They also include age, age-squared, gender, and years of schooling. The thin lines show the 95% confidence interval for the polynomial specification. Standard errors are clustered at the state-year-of-immigration-level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

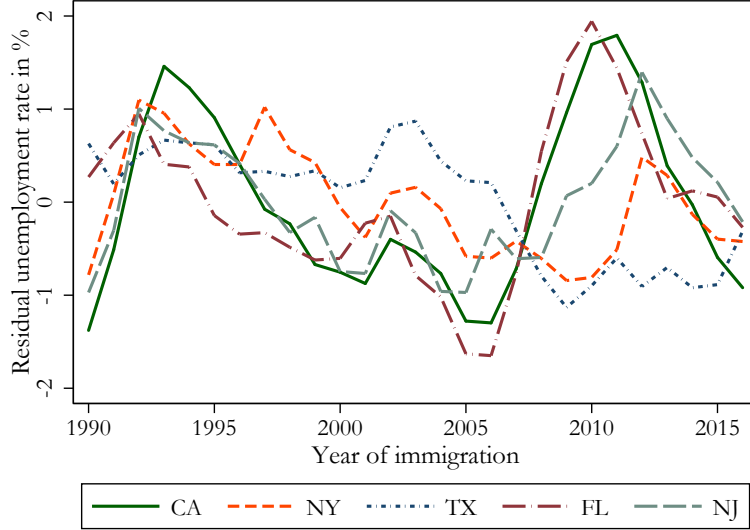
**Table B.4:** Correlation between official waiting time and difference in immigration year between spouses (F2As)

	Official waiting time				
	F1	F2A	F2B	F3	F4
Observed year difference	0.05	0.93	0.37	0.02	0.06

Notes: Based on repeated cross-section from the 2000 U.S. Census and 2000 to 2017 ACS. Sample restricted to spouses of immigrants between 22 and 60 years old at the time of observation. The coefficients are from a regression of the official waiting times for different categories on the observed difference in immigration years of migrant and subsequently immigrating spouse. The categories are F1 for unmarried sons and daughters of US citizens, F2A for spouses and children of legal permanent residents, F2B for unmarried adult children of legal permanent residents, F3 for married sons and daughters of US citizens and F4 for brothers and sisters of US citizens. The correlation between F2A and F2B is 0.57, therefore there is also a positive relationship with this category.

**Figure B.1:** Waiting times for family migrants from countries where per-country ceiling is binding by admission category

Notes: The graph shows waiting times for unmarried sons and daughters of US citizens (F1), spouses and children of legal permanent residents (F2A), unmarried adult children of legal permanent residents (F2B), married sons and daughters of US citizens (F3) and brothers and sisters of US citizens (F4). Data source: U.S. Department of State, own calculations.

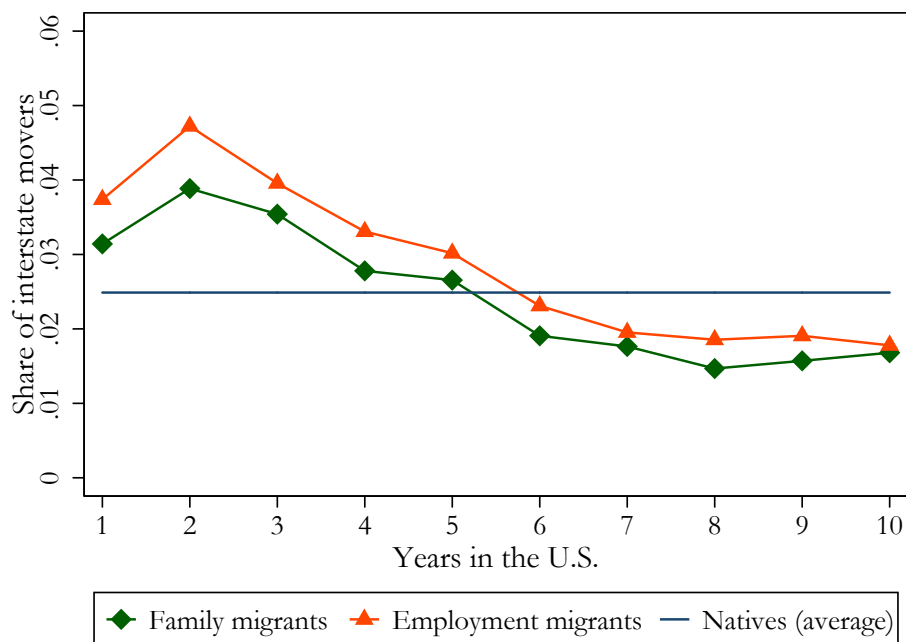
**Figure B.2:** State-level unemployment rate for most important migrant-receiving US states

Notes: State level unemployment rate is obtained from the Local Area Unemployment Statistics of the U.S. Bureau of Labor Statistics. The residual unemployment rate is estimated using state and year of immigration fixed effects. The graph displays the unemployment rate for the five most important immigration states for family migrants in our sample: California (CA), New York (NY), Texas (TX), Florida (FL) and New Jersey (NJ).

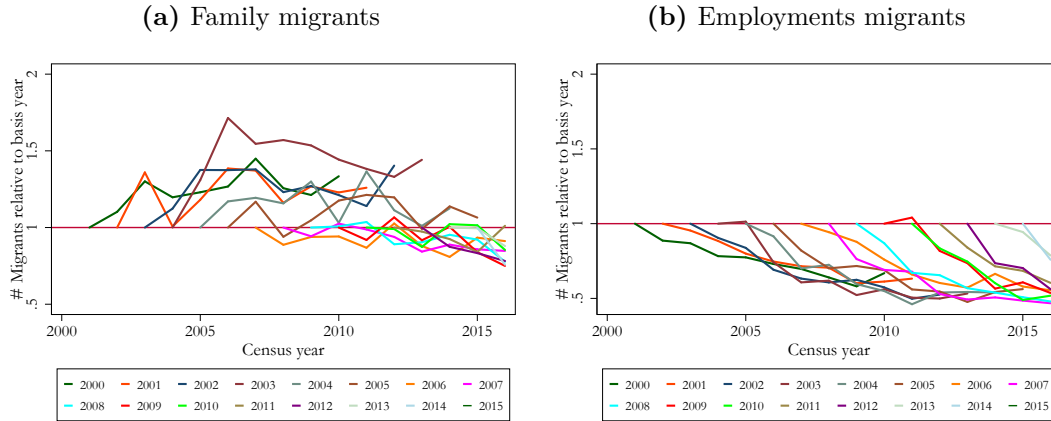
**Table B.5:** Effect of a one pp increase in initial unemployment rates by network strength for migrants from India

	Effect of initial conditions			Placebo		
	Employment	Wage income	% Indians in occ.	% natives in occ.	% Mexicans in occ.	% Chinese in occ.
UR at immigration	-0.005 (0.003)	0.004 (0.010)	0.192*** (0.045)	0.015 (0.015)	-0.013 (0.020)	0.068*** (0.026)
× medium network	0.003 (0.003)	-0.007 (0.007)	-0.088*** (0.032)	-0.016 (0.012)	-0.005 (0.016)	-0.045** (0.021)
× large network	0.008*** (0.003)	-0.016* (0.009)	-0.079** (0.037)	-0.009 (0.012)	-0.006 (0.017)	-0.063*** (0.021)
Observations	49983	34261	34261	34261	34261	34261

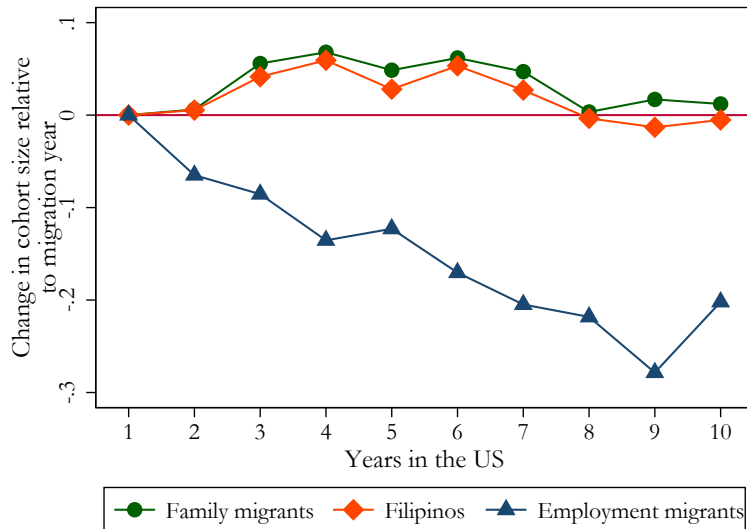
Notes: The analysis is based on data from the 2000 US Census and the 2001 to 2017 American Community Survey. The analysis is restricted to migrants from India that resided in the US for at most five years. See Section 2.4 for further details on the sample construction. Observations are weighted by year, i.e., the mean of person weights in the respective observation year. All specifications include dummies for years in the US, year of immigration, year of observation, and state of residence. They also include age, age-squared, gender, and years of schooling. The outcome variables in columns four to six are the share of workers in the respective group that works in the occupation (in percent). The variable is calculated at the national level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Figure B.3:** Share of individuals that moved between US states within the last year

Notes: The graph shows the share of individual that have moved between states in the last year. The analysis is based on a repeated cross-section based on the 2000 Census and the 2001 to 2016 ACS and restricted to individuals between 22 and 60 years old at the time of observation. It is further restricted to individuals born outside the U.S. or Philippines respectively who are between 2000 and years old at the time of migration and observed between 1 and 12 years after immigration.

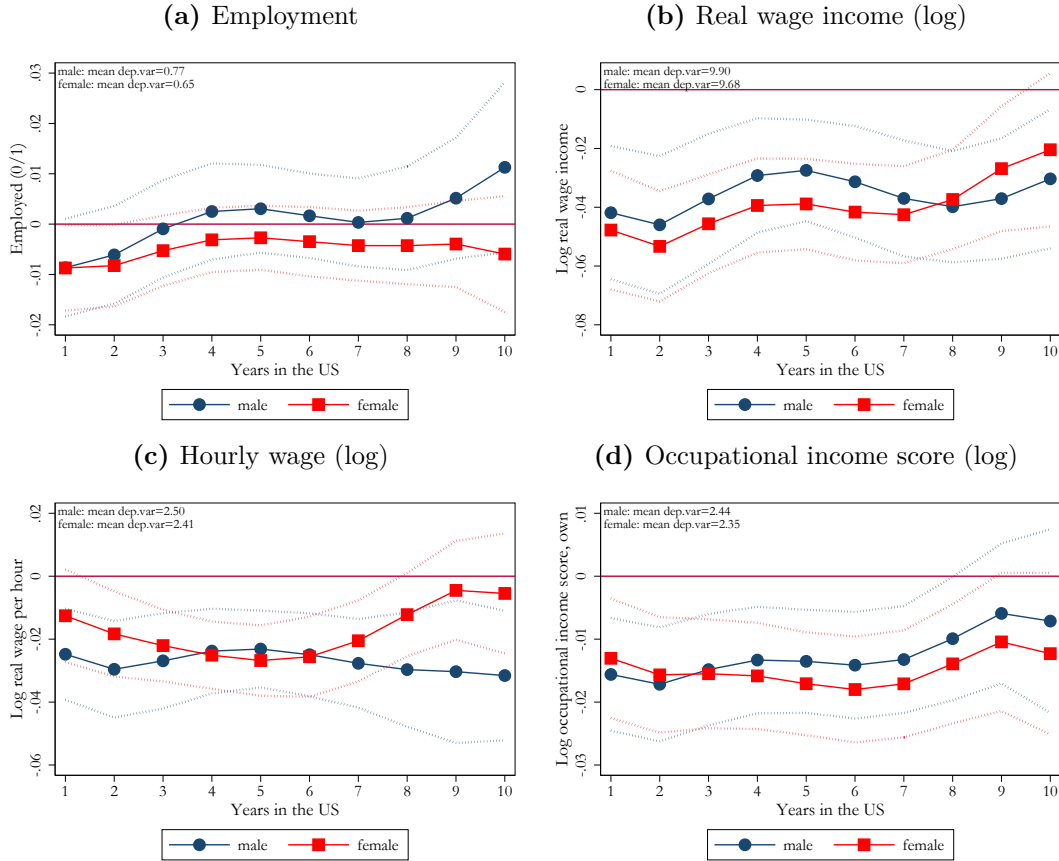
**Figure B.4:** Cohort size by years since immigration (individual years)

Notes: The analysis is based on data from the 2001 to 2017 American Community Survey. The figure depicts the analysis for two distinct samples: Family migrants (Panel a) and employment migrants (Panel b). Section 2.4 outlines the construction of these samples. The samples are restricted to individuals who were born outside the US as non-US citizens and immigrated between 2000 and 2016. We further restrict the sample to individuals 22 to 50 years old at the time of immigration and of observation because these immigrants remain in our sample for the full observation period. Observations are weighted by year, i.e., the mean of person weights in the respective observation year. The x-axis displays the observation year. The y-axis the number of migrants in the sample relative to the basis year.

**Figure B.5:** Cohort size by years since immigration (aggregated)

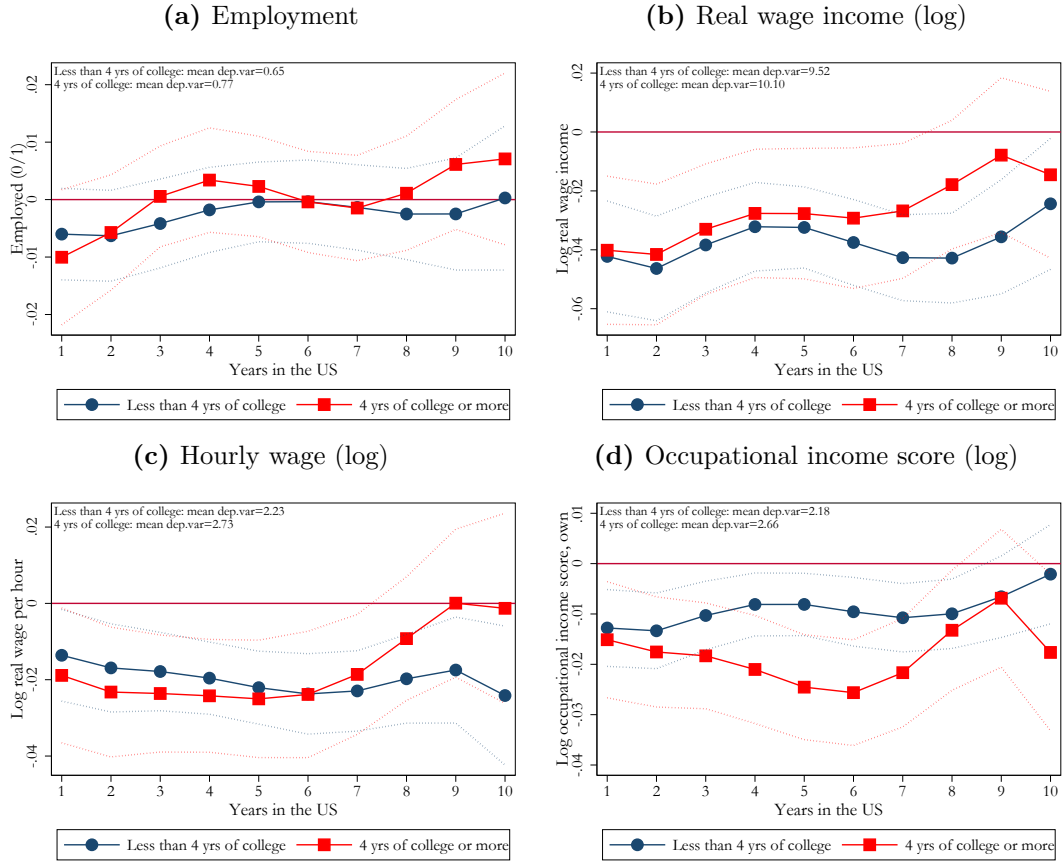
Notes: The figure plots the coefficients from a regression of the log number of observed immigrants on a full set of year-of-immigration and years-in-the-US dummies. The coefficients of the years-in-the-US dummies indicate the number of migrants observed in year  $t$  relative to the first year and thus capture potential return migration.

**Figure B.6:** Persistent effects of the unemployment rate at immigration on labor market outcomes for family migrants by sex



Notes: The analysis is based on data from the 2000 US Census and the 2001 to 2017 American Community Survey. The sample is restricted to individuals who were born outside the US as non-US citizens and immigrated between 1990 and 2016. We further restrict the sample to individuals 22 to 60 years old at the time of immigration and of observation. To identify family migrants, we restrict to the countries of origin with mostly family migration as outlined in Section 2.4. Observations are weighted by year, i.e., the mean of person weights in the respective observation year. The x-axis displays the years since migration. The y-axis shows the effect of a one pp higher state-level unemployment rate at immigration on the outcome variable in the respective year. The graph shows the marginal effects using the polynomial specification from Equation (2.2) for males (blue dots) and females (red squares). Both specifications include dummies for years in the US, year of immigration, year of observation and state of residence. They also include age, age-squared, gender, and years of schooling. The thin lines show the 95% confidence interval for the polynomial specification. Standard errors are clustered at the state-year-of-immigration-level.

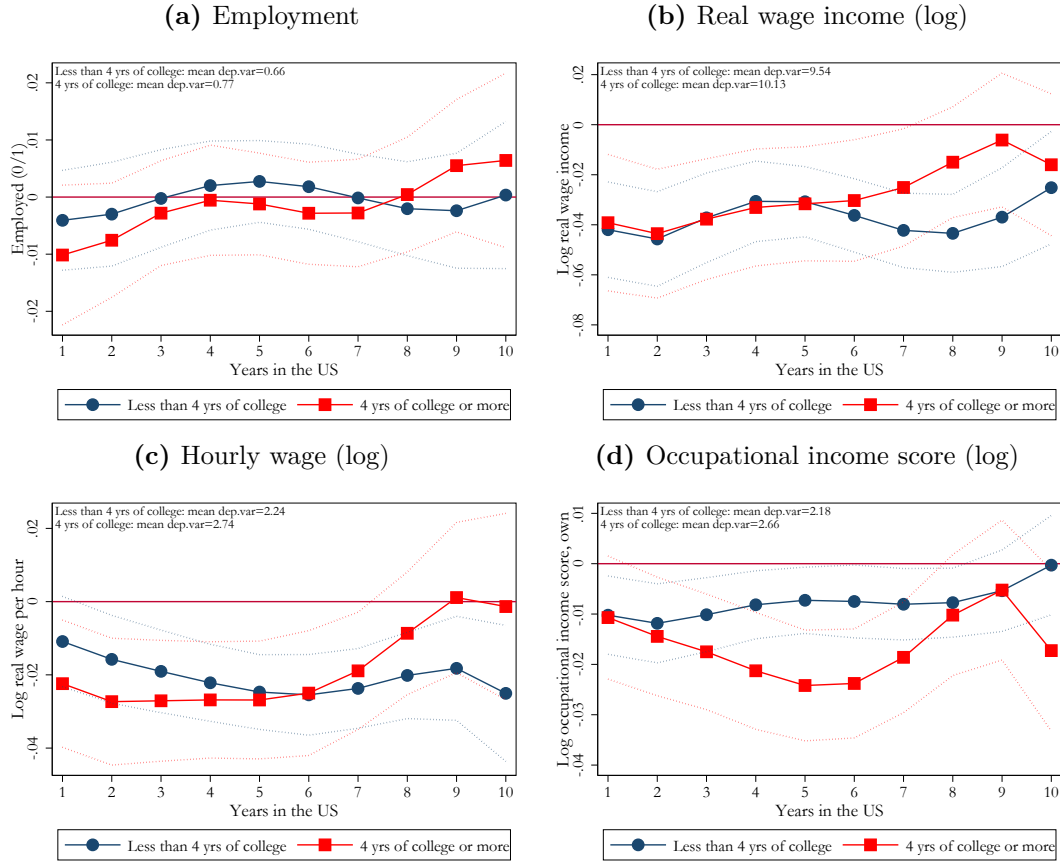
**Figure B.7:** Persistent effects of the unemployment rate at immigration on labor market outcomes for family migrants by education



Notes: The analysis is based on data from the 2000 US Census and the 2001 to 2017 American Community Survey. The sample is restricted to individuals who were born outside the US as non-US citizens and immigrated between 1990 and 2016. We further restrict the sample to individuals 22 to 60 years old at the time of immigration and of observation. To identify family migrants, we restrict to the countries of origin with mostly family migration as outlined in Section 2.4. Observations are weighted by year, i.e., the mean of person weights in the respective observation year. The x-axis displays the years since migration. The y-axis shows the effect of a one pp higher state-level unemployment rate at immigration on the outcome variable in the respective year. The graph shows the marginal effects using the polynomial specification from Equation (2.2) for individuals with less than four years of college (blue dots) and four years of college or more (red squares). Both specifications include dummies for years in the US, year of immigration, year of observation and state of residence. They also include age, age-squared, gender, and years of schooling. The thin lines show the 95% confidence interval for the polynomial specification. Standard errors are clustered at the state-year-of-immigration-level.

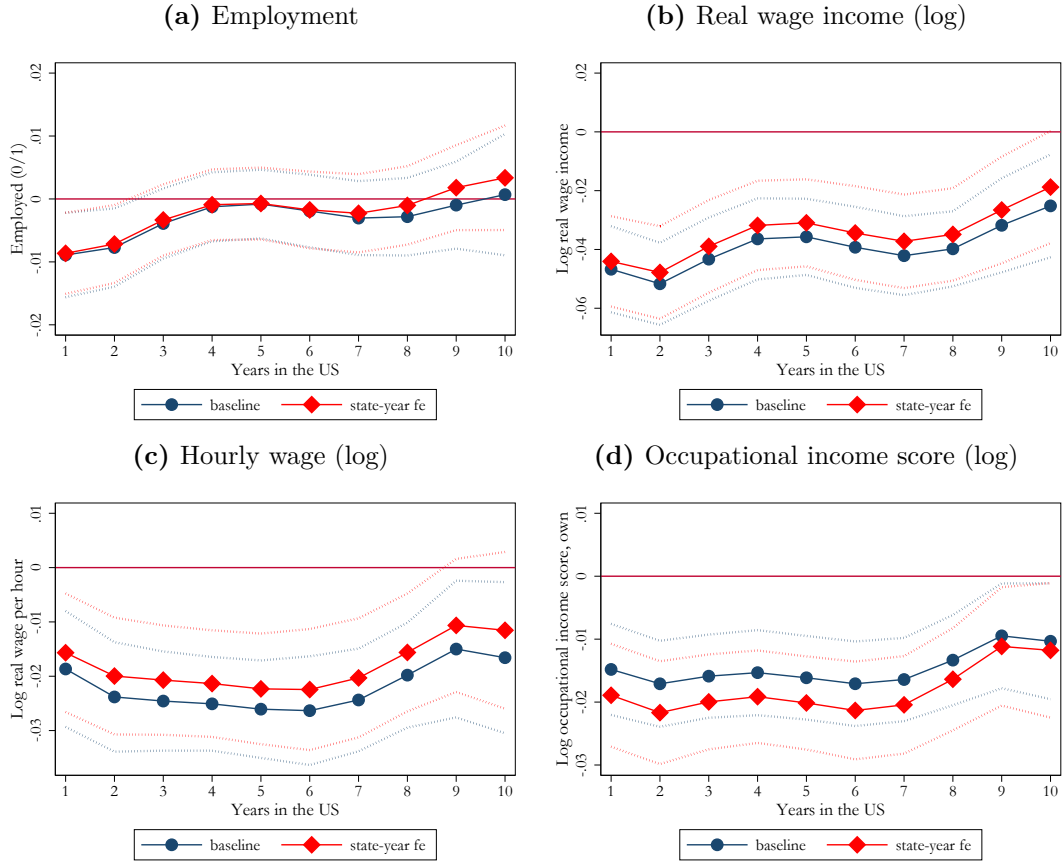


**Figure B.8:** Persistent effects of the unemployment rate at immigration on labor market outcomes for family migrants by education (cond. on age at immigration larger than 30)



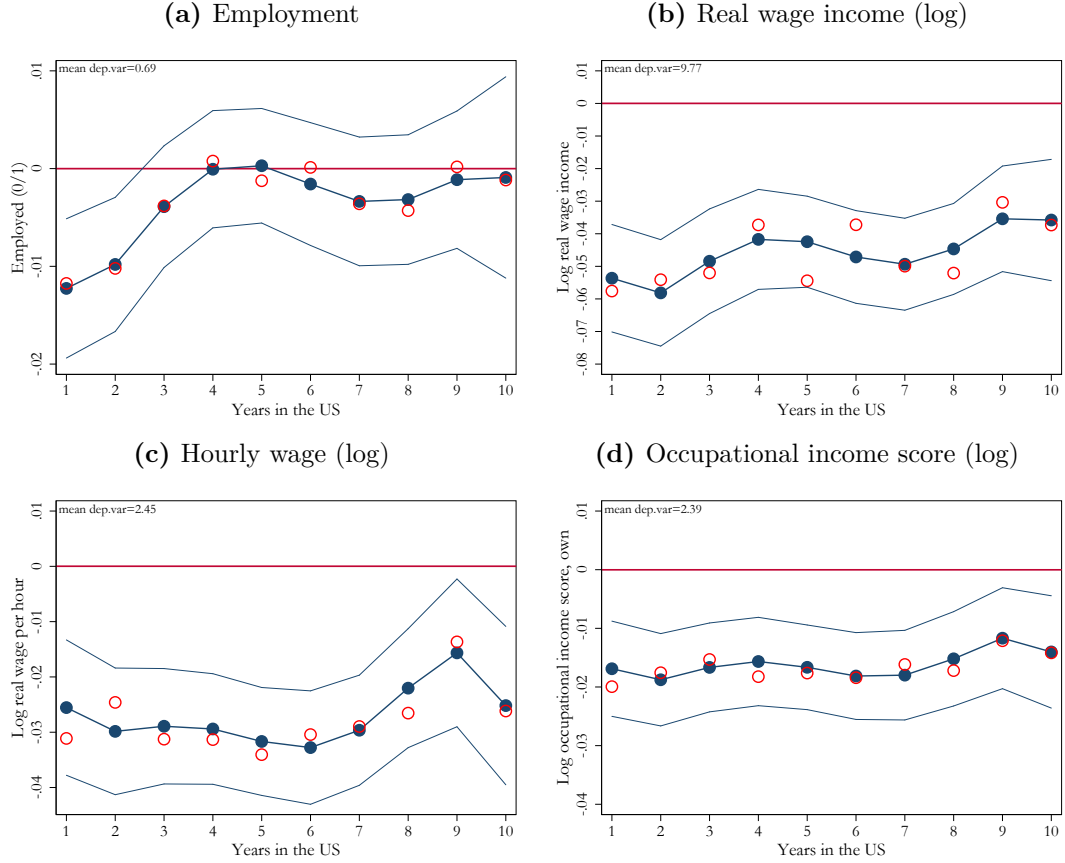
Notes: The analysis is based on data from the 2000 US Census and the 2001 to 2017 American Community Survey. The sample is restricted to individuals who were born outside the US as non-US citizens and immigrated between 1990 and 2016. We further restrict the sample to individuals 30 to 60 years old at the time of immigration and of observation. To identify family migrants, we restrict to the countries of origin with mostly family migration as outlined in Section 2.4. Observations are weighted by year, i.e., the mean of person weights in the respective observation year. The x-axis displays the years since migration. The y-axis shows the effect of a one pp higher state-level unemployment rate at immigration on the outcome variable in the respective year. The graph shows the marginal effects using the polynomial specification from Equation (2.2) for individuals with less than four years of college (blue dots) and four years of college or more (red squares). Both specifications include dummies for years in the US, year of immigration, year of observation and state of residence. They also include age, age-squared, gender, and years of schooling. The thin lines show the 95% confidence interval for the polynomial specification. Standard errors are clustered at the state-year-of-immigration-level.

**Figure B.9:** Persistent effects of the unemployment rate at immigration on labor market outcomes controlling for contemporaneous conditions



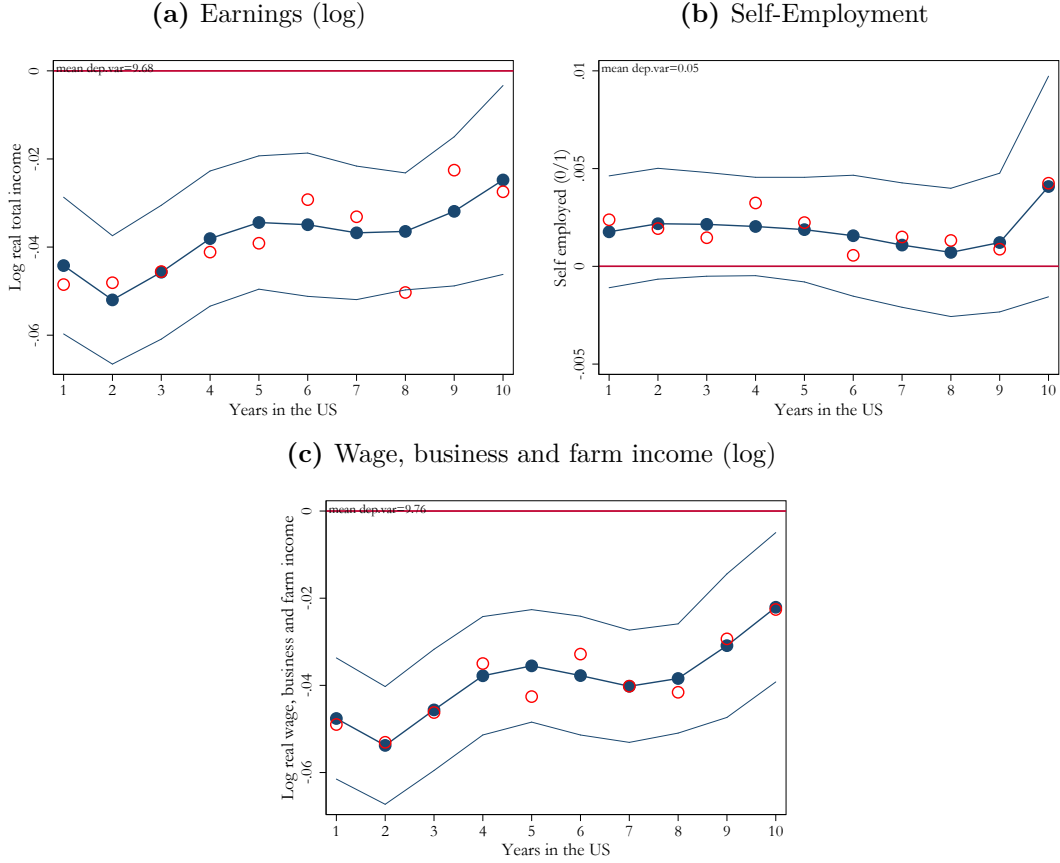
Notes: The analysis is based on data from the 2000 US Census and the 2001 to 2017 American Community Survey. The sample is restricted to individuals who were born outside the US as non-US citizens and immigrated between 1990 and 2016. We further restrict the sample to individuals 22 to 60 years old at the time of immigration and of observation. To identify family migrants, we restrict to the countries of origin with mostly family migration as outlined in Section 2.4. Observations are weighted by year, i.e., the mean of person weights in the respective observation year. The x-axis displays the years since migration. The y-axis shows the effect of a one pp higher state-level unemployment rate at immigration on the outcome variable in the respective year. The graph shows the marginal effects using the polynomial specification from Equation (2.2) for the baseline specification (blue dots) and including state-year fixed effects (red diamonds). Both specifications include dummies for years in the US, year of immigration, year of observation and state of residence. They also include age, age-squared, gender, and years of schooling. The thin lines show the 95% confidence interval for the polynomial specification. Standard errors are clustered at the state-year-of-immigration-level.

**Figure B.10:** Persistent effects of the average unemployment rate in the first three years since immigration on labor market outcomes



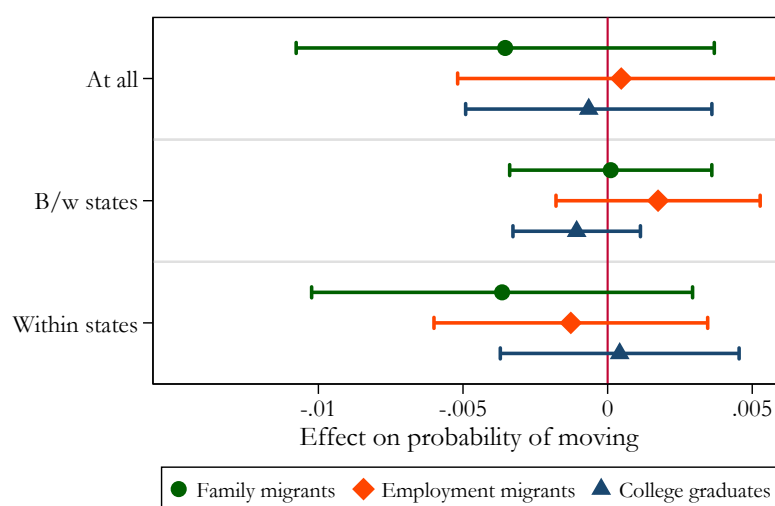
Notes: The analysis is based on data from the 2000 US Census and the 2001 to 2017 American Community Survey. The sample is restricted to individuals who were born outside the US as non-US citizens and immigrated between 1990 and 2016. We further restrict the sample to individuals 22 to 60 years old at the time of immigration and of observation. To identify family migrants, we restrict to the countries of origin with mostly family migration as outlined in Section 2.4. This leaves us with a sample of 88,357 migrants overall and out of which 65,565 are employed. Observations are weighted by year, i.e., the mean of person weights in the respective observation year. The x-axis displays the years since migration. The y-axis shows the effect of a one pp higher state-level average unemployment rate in the first three years since immigration on the outcome variable in the respective year. The graph shows the result of two different specifications. The red hollow circles show the effect using the flexible specification from Equation 2.1. The connected blue dots show the marginal effects using the polynomial specification from Equation 2.2. Both specifications include dummies for years in the US, year of immigration, year of observation, and state of residence. They also include age, age-squared, gender, and years of schooling. The thin lines show the 95% confidence interval for the polynomial specification. Standard errors are clustered at the state-year-of-immigration-level.

**Figure B.11:** Persistent effects of the unemployment rate at immigration on self employment, business and farm income, and total earnings



Notes: The analysis is based on data from the 2000 US Census and the 2001 to 2017 American Community Survey. The sample is restricted to individuals who were born outside the US as non-US citizens and immigrated between 1990 and 2016. We further restrict the sample to individuals 22 to 60 years old at the time of immigration and of observation. To identify family migrants, we restrict to the countries of origin with mostly family migration as outlined in Section 2.4. This leaves us with a sample of 88,357 migrants overall and out of which 65,565 are employed. Observations are weighted by year, i.e., the mean of person weights in the respective observation year. The x-axis displays the years since migration. The y-axis shows the effect of a one pp higher state-level unemployment rate at immigration on the outcome variable in the respective year. The graph shows the result of two different specifications. The red hollow circles show the effect using the flexible specification from Equation 2.1. The connected blue dots show the marginal effects using the polynomial specification from Equation 2.2. Both specifications include dummies for years in the US, year of immigration, year of observation, and state of residence. They also include age, age-squared, gender, and years of schooling. The thin lines show the 95% confidence interval for the polynomial specification. Standard errors are clustered at the state-year-of-immigration-level.

**Figure B.12:** Effect of the unemployment rate at immigration on geographic mobility in the first five years after immigration



Notes: The analysis is based on data from the 2000 US Census and the 2001 to 2017 American Community Survey. The displays the results for family migrants, employment migrants and college graduates. See Section 2.4 for further details on the sample construction. The outcome variables are whether individuals moved at all, moved between states and moved within states. Observations are weighted by year, i.e., the mean of person weights in the respective observation year. All specifications include dummies for years in the US, year of immigration, year of observation, and state of residence. They also include age, age-squared, gender, and years of schooling.  $*p < 0.1$ ,  $**p < 0.05$ ,  $***p < 0.01$ .



## Chapter 3

# The Political Economy of Immigrant Legalization: Evidence from the 1986 IRCA<sup>\*</sup>

### Abstract

What happens to the distribution of public resources when undocumented migrants obtain legal status through nation-wide amnesty? In this paper, we exploit variation in legal status from the 1986 Immigration Reform and Control Act (IRCA) to answer this question and find that state governors, of whatever party affiliation, allocate more per capita aid to those counties affected by the IRCA. We posit that this is borne out of rational, forward-looking governors who allocate resources strategically in pursuit of the votes of the newly legalized who were eligible to vote five years after legalization. To support this view, we find that the distribution of state aid differs significantly according to political context. Counties affected by the IRCA receive more resources from the state when their governor is eligible for re-election, faces political competition or enjoys line-item veto power. Our results also indicate that the transfers were targeted to the newly legalized, who by and large were of Hispanic origin, and not other constituents. We find no evidence of anti-migrant sentiment confounding our results. Counties that received more transfers from the governor also experienced improvements in Hispanic high school completion rates.

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<sup>\*</sup> This chapter is based on joint work with Navid Sabet.

### 3.1 Introduction

Undocumented migration has today become a hotly contested issue. This contest is, perhaps, fiercer nowhere than in the United States where, as of 2016, an estimated 11.3 million migrants reside in the country illegally, up from 5.8 million in 1996.<sup>1</sup> Given the polarized nature of the issue, we ask in this chapter what happens to the distribution of state and local finances when undocumented migrants are offered legal status through a nation-wide amnesty. We answer the question by focusing on the actions of incumbent state governors: how do they respond, in terms of public resource allocation, to the incentives created by documenting undocumented migrants? Do they allocate more public resources to the areas affected by amnesty and to what extent is such allocation intended to capture the votes of the newly documented migrants as opposed to other, perhaps competing, constituents?

The history of the United States offers a unique opportunity to study these issues. In 1986, the Reagan Administration passed into law the Immigration Reform and Control Act (IRCA) which legalized nearly 3 million undocumented migrants in the span of some three years and offered them a path to citizenship five years after legalization. In this study, we combine variation in legal status from the IRCA with administrative data from the Census of Governments in order to throw light on the ways in which documenting undocumented migrants affects the distribution of state and local finances. Our analysis is motivated by a model of distributive politics in which incumbent politicians react to changes in the electorate by adjusting their budget allocations to target the preferences of the newly enfranchised group so as to optimize (a) the welfare of the population and (b) their own re-election chances. Accordingly, we posit that governors, who play an important role in formulating the state budget, will allocate resources strategically to those counties most affected by the IRCA in an effort to win the political support of the newly documented migrants who were eligible to vote five years after legalization.

Using a difference-in-differences regression framework, we compare the distribution of public finances—specifically, per capita inter-governmental transfers from state to local governments—in counties that experienced more per capita legalizations with those that received less both before and after 1986. Our baseline estimate suggests that counties with a greater share of IRCA-documented migrants received more per capita transfers from their state governments than those with a fewer share of such migrants. The result is not driven by differences in county social, economic or demographic characteristics and is robust to alternative specifications and samples.

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<sup>1</sup> Pew Research Centre, taken from <http://pewrsr.ch/2oWlM93>. Accessed 9 February 2018.



To overcome potential geographical endogeneity associated with where undocumented migrants settle, we follow two approaches: first, we employ propensity score matching to identify a more comparable control group and second, we use the share of a county's 1960 population that is foreign-born as an instrument for the number of documented migrants per county post-1986. These tests confirm that the baseline result is not confounded by geographical factors.

Arguably, our result may be driven by mechanical or bureaucratic forces that oblige the state governor to better service the areas where the newly documented migrants reside rather than, as we posit, discretionary forces borne out of political calculation. To refute this competing explanation we analyze, in the second part, the sensitivity of inter-governmental transfers to political constraints. The rationale is straightforward. If the transfers of the state government are reflective of forces outside the control of the governor, then the result ought to be insensitive to political constraints faced by the governor. If, on the other hand, the transfers are politically motivated, the result ought to exhibit heterogeneity with respect to the various political constraints facing the incumbent. Consistent with this line of thinking, we find that counties affected by the IRCA receive more resources from the state when their governor is eligible for re-election, faces political competition, enjoys line-item veto power over the budget or is politically aligned with the state legislature. We also uncover heterogeneity along party lines: although both Republican and Democratic governors allocate significantly more resources to IRCA-affected counties, the effect increases by about half when the governor is a Democrat. It is, perhaps, unsurprising, therefore, that we also find that a governor's likelihood for re-election increases in the share of newly documented migrants in a state.

Finally, the IRCA provided a path to citizenship five years after legalization. How plausible is it, then, that governors target their transfers to actually meet the needs, and win over the future political support, of the newly legalized as opposed to other, perhaps competing, constituents in a county? To address this issue, we first undertake a number of empirical checks to alleviate concerns that anti-migrant sentiment may drive our results. In this respect, we exploit data on a prominent anti-migrant ballot measure in California and find no relationship between state transfers and the interaction between a county's share of legalized migrants and its support for the ballot measure. More generally, we utilize survey data from the entire United States and find that, if anything, areas more affected by the IRCA actually experience improvements in attitudes towards both documented and undocumented migrants, further suggesting that anti-migrant sentiment is not confounding our results. Next, we turn our attention to local expenditure and find that expenditure in education increases in the

share of newly documented migrants in a county. Hispanic individuals, as opposed to Caucasian ones, residing in these counties and who entered middle school after 1986 experience improvements in the likelihood of completing high school, suggesting that the additional resources a county receives on account of the IRCA were in fact targeted to the newly legalized as opposed to other voting blocs.

Together, these results point to a strong, nuanced political economy facet of immigrant legalization. Although models of distributive politics generally predict that the expansion of franchise leads to greater resources being allocated to those who have a new-found political voice, it is more of an open question to what extent politicians allocate resources to capture this new “swing” vote as opposed to further solidify a core constituency. Our results indicate that, at least in the context of undocumented migration, politicians allocate resources primarily in an effort to capture the political support of the new swing vote rather than that of other groups.

In this chapter, we offer two contributions. First, we contribute to the literature that sheds light on the distributional effects of the expansion of voter franchise. Cascio and Washington (2014), for example, study enfranchisement of African Americans in the United States through the Voting Rights Act of 1965. They find that counties that removed literacy tests at voter registration in response to the law experienced greater voter turnout among black voters which, in turn, increased the share of public spending directed towards them. In a similar vein, Miller (2008) shows that the enfranchisement of women in the United States was followed by immediate changes in legislative behavior and substantial increases in public health spending at the local level. Examining the impact of electronic voting technology in Brasil, Fujiwara (2015) finds that the enfranchisement of lesser educated citizens affected government spending and increased health care spending, both of which are particularly beneficial for lower-income people. Naidu (2012), looking at a case of disenfranchisement, analyses the effects of the introduction of poll taxes and literacy tests in the 19<sup>th</sup> century United States and finds that such measures lowered overall voter turnout with the subsequent effect of worse educational outcomes for black pupils and losses in annual income in the order of 15 percent for black laborers. We extend this literature to consider the case of undocumented Hispanic migrants in the United States, one of the largest disenfranchised groups in the country. Moreover, because of our rich data on state governors, we are able to examine some of the political mechanisms that lead governors to allocate resources in light of a sudden shock to the electorate.

Second, we contribute to the literature on the economics of legal status. In this respect, the IRCA has been used as a credible policy shock to identify the impact of legal

status on various social and economic outcomes at the level of the individual migrant.<sup>2</sup> For example, Cortes (2013) shows that legal status helps migrants to obtain better educational outcomes whilst Kossoudji and Cobb-Clark (2002) and Pan (2012) use the IRCA to show that documenting undocumented migrants leads to an improvement in their wages, employment prospects and ability to speak English. We contribute to this literature by examining the influence of legal status directly on the distribution of public resources at the state and local level, bringing to light a dimension of undocumented migration that is highly debated yet relatively understudied.

The rest of this chapter proceeds as follows: Section 3.2 contextualizes the study by discussing the historical background of the IRCA as well as the demographic characteristics of its applicants. In Section 3.3 we present a simple framework that guides our empirical analysis and the interpretation of our findings. Section 3.4 describes our data and explains institutional features associated with the budget-making process at the state level. Section 3.5 outlines our econometric methodology and, along with sections 3.6 and 3.7, presents our results. Section 3.8 concludes.

## 3.2 Background

### 3.2.1 The Immigration Reform and Control Act

The Immigration Reform and Control Act (IRCA) of 1986 was, to date, the most extensive piece of legislation put forward by the United States government to address the question of undocumented immigration. The passage of the IRCA was by no means straightforward. It began in the 1970s when the legislative and executive branches of government considered various elements of comprehensive immigration reform. These efforts gained momentum when, in 1977, Congress appointed the Select Commission on Immigration and Refugee Policy which presented, in 1981, a proposal for immigration reform which was ultimately rejected. In the years that followed, several other proposals were put forward and variants of the IRCA were passed through either the Senate or the House but none was able to win complete approval until the 99<sup>th</sup> Congress settled on and approved the IRCA on 17 October 1986 and which was signed into law on 6

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<sup>2</sup> Although some studies consider the impact of legal status on more aggregate outcomes. Baker (2015), for example, finds that counties with greater shares of IRCA applicants experienced a decline in crime rates.

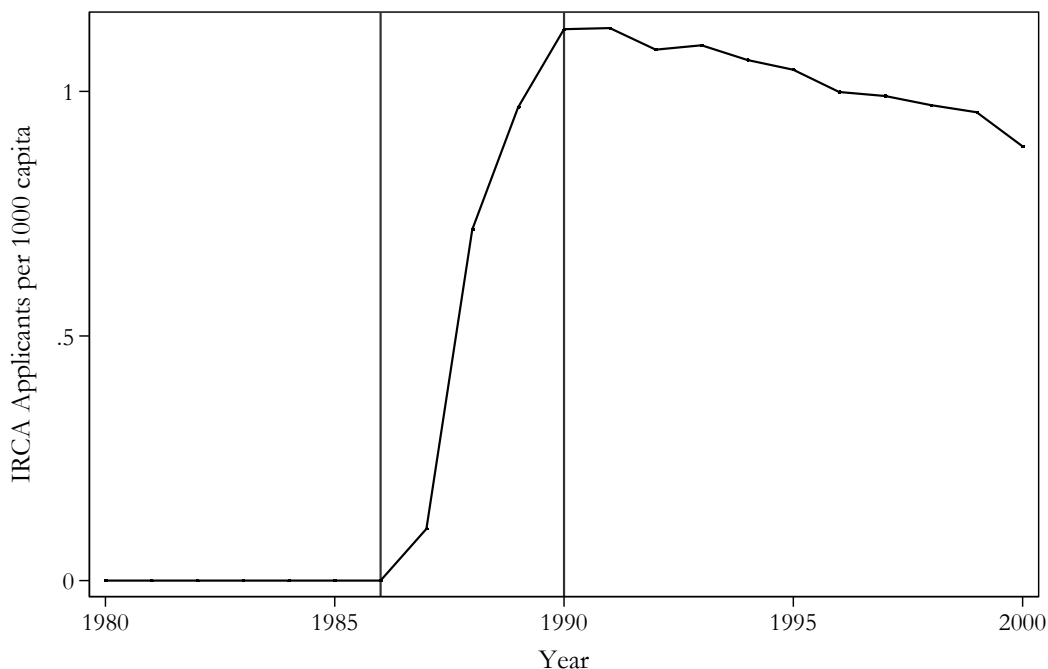
November 1986.<sup>3</sup>

The purpose of the IRCA was to restrict the flow of undocumented migrants into the United States. It rested on three main pillars: an employer sanctions provision that made it illegal for employers to knowingly hire unauthorized workers; increased funding for border security to discourage new people from migrating illegally; and an amnesty program intended to legalize various unauthorized workers (Chishti and Kamasaki, 2014).

While each of the components of the law was not without problems (in particular the employer sanction scheme which led to a great amount of fraud as well as workplace discrimination), the legalization program is generally regarded as the law's most successful provision. It provided two programs for two distinct groups of unauthorized workers. First, the Legally Authorized Workers (LAWs, also known as "pre-82s") under section 245A of the law enabled undocumented immigrants who resided in the country for an uninterrupted period from before 1 January 1982 to legalize (DHHS, 1991; Cascio and Lewis, 2017). Second, the Special Agricultural Workers (SAW) under Section 210 of the law allowed applications from unauthorized migrants who could show that they carried out 90 days of work on select USDA defined seasonal crops in the year leading to 1 May 1986 (DHHS, 1991; Cascio and Lewis, 2017). LAW applicants were eligible to apply within a 12-month time frame extending from May 1987 to May 1988 whereas SAW applicants had an 18-month application period from 1 June 1987 to 30 November 1988 (DHHS, 1991). On acceptance of their application, applicants were given temporary legal status under the title of *Temporary Resident Aliens* which could last for as long as 18 months. After this period, and upon successful completion of an English test and a civics test, applicants were given permanent resident status.

At the time of the Act, there were some 3 million undocumented immigrants residing in the United States, corresponding to nearly 1 percent of the population (Wasem, 2012). The law stipulated that both application periods (the 12 months for the LAW program and 18 months for the SAW program) were strictly enforced, which from an econometric point of view implies a relatively clean identification period. Indeed, by the end of the application period, roughly 3 million people applied for temporary resident status, of which 1.7 million comprised LAWs and 1.3 million comprised SAWs (DHHS, 1991). By 1990, 94.6 percent and 58.7 percent, respectively, of LAW and SAW

<sup>3</sup> The timing of the IRCA's passage in 1986 was indeed sudden and unexpected. Just days before its passage in Congress, "congressional leaders pronounced it dead, this time after more than fifteen months of hearings, legislative negotiations and debate" (Fuchs, 1990). Speaking to this idea, Representative Daniel E. Lungren (R-California) remarked on the day of the bill's passage that the IRCA was "a corpse going to the morgue, and on the way to the morgue a toe began to twitch and we started CPR again" (Fuchs, 1990). See Table C.1 for details on how Congress voted to pass the bill.

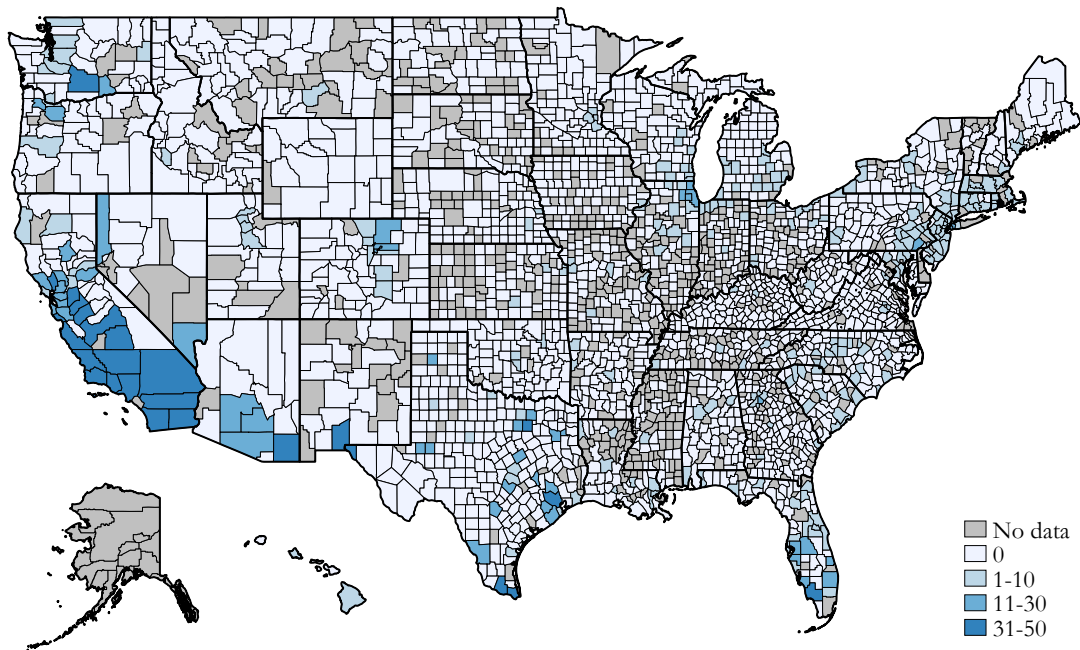
**Figure 3.1:** Stock of IRCA applicants at the county level per 1,000 capita

applications had received approval and for all intents and purposes, the legalization program of the IRCA ceased to generate newly legalized citizens after 1990 (DHHS, 1991). Figure 3.1 shows the time trend of the stock of IRCA legalized migrants while Figure 3.2 shows the geographic distribution of IRCA applicants at the county level in 1992 for those counties for which data is available.<sup>4</sup> As shown, the majority of legalizations took place between 1986 and 1990 and in the states of California (970,895), Texas (351,646), Illinois (125,399), Arizona (70,488) and New Jersey (29,012). As shown in Figure 3.3, undocumented migrants applied for legal status in approximately 330 counties whereas the remaining counties received no such applications.

### 3.2.2 Demographic Characteristics of the Legalized

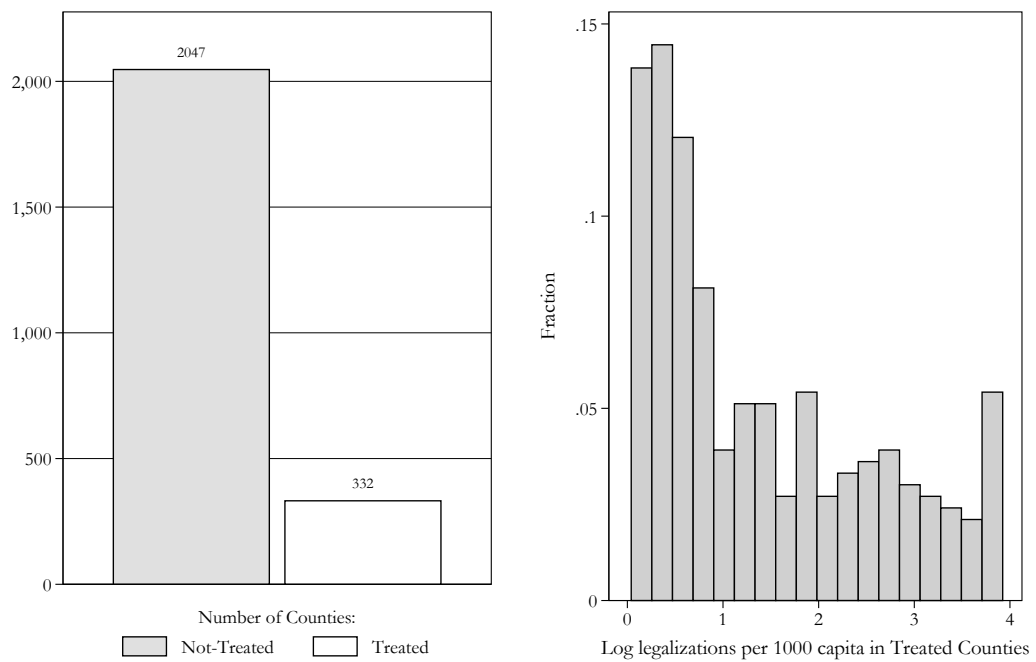
Figure 3.4 presents data from the December 1991 report to Congress from the Department of Health and Human Services which documented the characteristics of the

<sup>4</sup> Of the 3,142 counties in the United States, our dataset includes IRCA information on 2,760 of them (and from all states except Alaska and Delaware). However, we do not observe every county in every year because some of the counties drop out in the later stages of the sample. As such, the actual number of treated counties varies slightly in the sample from 276 counties in 1999 to 332 in 1991/92. Restricting the analysis to only those counties that we observe throughout the entirety of the sample makes no difference to the results.

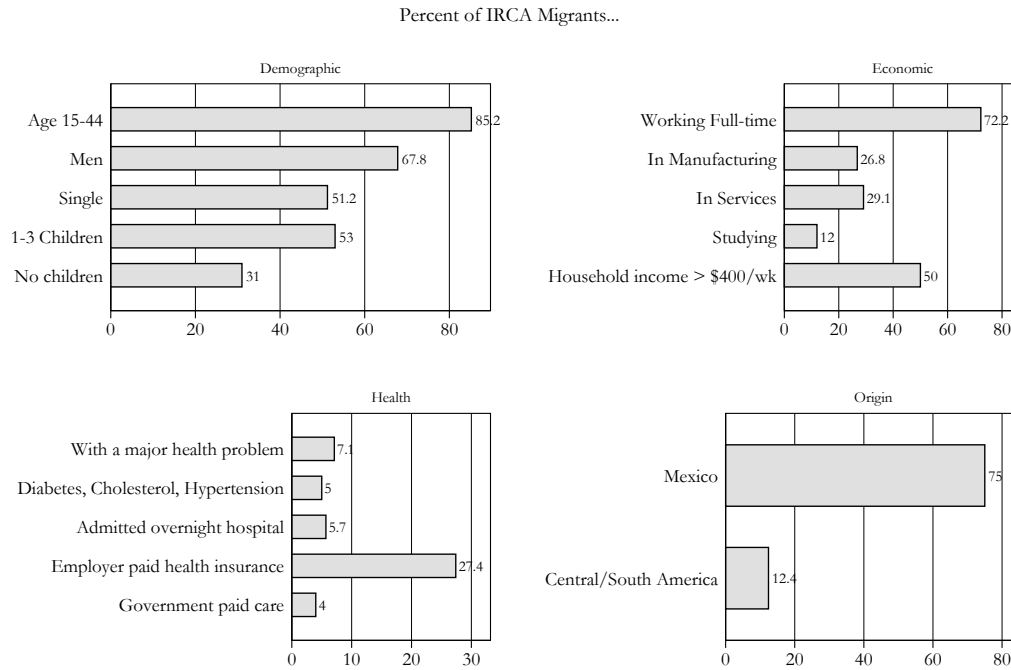
**Figure 3.2:** Number of legalized migrants per 1,000 county inhabitants in 1992

legalized population. This data indicates that the newly legalized are predominantly of working age, healthy and with relatively few children. More than half and two-thirds, respectively, are single and male and the vast majority of applicants were engaged in full-time work. Fully 22 percent of all applicants reported a household income of over \$600 per week; well over the poverty line, which, in 1989 stood at \$6,311 for a single person (\$121 per week) and \$12,675 for a family of four (\$244 per week) (Mosbacher and Bryant, 1991).<sup>5</sup> In fact, median take-home pay for IRCA applicants stood at \$400 per week. Median household income in the population in 1989 stood at \$23,745, or \$456 per week. The report also makes clear that no more than 5 percent of the migrants reported not being able to work in the month prior to the survey. As such, IRCA applicants were, by and large, an economically active and self-reliant group earning somewhere between the poverty threshold and median income.

<sup>5</sup> The National Longitudinal Survey of the U.S. Bureau of Labor Statistics suggests the poverty thresholds in 1989 were even lower: \$5,980 for a single person and \$12,100 for a family of four. Taken from <http://bit.ly/2tGnz8V>. Accessed in August 2017.

**Figure 3.3:** Distribution of legalized migrants

Notes: This graph plots the distribution of the legalized migrants in 1992. The left panel indicates the number of treated (332) and control (2,047) counties whereas the panel on the right plots the distribution of treatment within treated counties. On a linear scale, the average treated county experienced 4.4 applications per 1,000 county inhabitants. The average population size of treated counties is just over 409,490, implying that on average 1,800 migrants received legal status in treated counties.

**Figure 3.4:** Socio-economic characteristics of the IRCA applicants

Notes: These are the characteristics of the IRCA migrants as reported by Congress in 1991.

Source: DHHS (1991)

### 3.3 Incumbent Politicians: A Framework

In this section, we present a simple framework to guide our empirical analysis. In this framework, an incumbent politician controls the distribution of transfers flowing from the state budget to the various counties in the state. The politician is concerned both with the welfare of the population and her own re-election. A sudden change of legal status in a large and homogeneous group of residents in a county will thus change the politician's decision on how to distribute state resources so as to optimize her re-election chances and the welfare of the population. In our setting, therefore, we expect that the legalization of a large group of mostly Hispanic people of lower socio-economic status will prompt politicians to offer policy over public resources so as to target the preferred outcome of the newly legalized group in the hope of winning



their future political support.<sup>6</sup>

### 3.3.1 Politician Pay-off

We begin by modeling the objective function of an incumbent politician,  $P$ . In doing so, we adapt a model presented by Englmaier and Stowasser (2017) to consider a case of a state governor. For every county in the state,  $P$  transfers an amount of government assistance,  $g > 0$ , to the population at cost  $C(g)$ . As mentioned above,  $P$  is concerned with the utility of the residents,  $U$ , in every county as well as the utility derived from her expected vote share in elections,  $\Omega$ .

Each county is composed of two types of inhabitants: already legal citizens,  $C$ , and newly legalized migrants,  $L$ . The population in each county is normalized to one such that the share of the population that is newly legalized is  $\alpha$  and the share of already legal citizens is  $1 - \alpha$ . Only citizens can vote and voting decisions are based on local economic conditions, such as government expenditure on various public services. Voters base their voting decisions not just on local conditions immediately prior to the election but throughout  $P$ 's term.

For every county in the state,  $P$  has a pay-off,  $\Pi$ , that is composed of three parts, shown in Equation (3.1):

$$\Pi = \underbrace{(1 - \alpha)U_C(g) + \alpha U_L(g)}_1 + \underbrace{E \cdot \Omega[\phi((1 - \alpha)U_C(g) + \alpha U_L(g))]}_2 - \underbrace{C(g)}_3 \quad (3.1)$$

The politician is concerned with the well being of the population in each county. This is reflected in the first term of  $P$ 's pay-off,  $(1 - \alpha)U_C(g) + \alpha U_L(g)$ . We assume utility functions are concave such that  $U'_i(g) > 0$  and  $U''_i(g) < 0 \forall i \in \{C, L\}$ . The only way in which  $P$  can improve the utility of the population is through her allocation of  $g$ . Because the characteristics of the documented migrants presented earlier, we take it as given that  $U'_L(g) > U'_C(g) \forall g$ .

We assume there are gains to staying in office. Accordingly, the second term,

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<sup>6</sup> In theory, there are two channels through which a policy shock such as the IRCA can affect the distribution of public resources (Persson and Tabellini, 2000). On the one hand, it might prompt a distribution of resources that is broad and general, for example, social benefits to all the members of some broadly defined group, such as the unemployed or the elderly. On the other, such programs may be targeted and specific, aiming to benefit a more narrowly defined subgroup of the population. Broad redistributive programs are those that appeal to the majority of the electorate and reflect their policy preferences. More targeted programs, by contrast, impose little cost on the majority of the electorate but offer great incentives for its beneficiaries. Although state tax revenue does increase with the share of documented migrants in a state, the tax rate does not increase as a function of IRCA applicants and we show results later in the paper that suggest the transfers IRCA-affected counties receive from the state are, in fact, used for targeted local expenditures.

$E \cdot \Omega[\phi((1 - \alpha)U_C(g) + \alpha U_L(g))]$ , captures the pay-off  $P$  obtains from re-election.  $E$  is a binary variable that is one when  $P$  is eligible for re-election and zero when  $P$  is a lame duck. In every election for which  $P$  is eligible to run,  $\phi$  captures her expected vote share in that election which is a function of the well being of the population. We assume that  $\phi$  is a linear function bounded between zero and one. The utility  $P$  derives from this expected vote share is captured by  $\Omega$ . It is assumed that  $\Omega(\phi)$ , a strictly increasing, non-linear function with a negative third order derivative. Figure C.1 provides an illustration of what such a function might look like. As shown, the marginal utility derived from the expected vote share is the greatest at the inflection point of  $\Omega(\phi = \phi_T)$  which represents the winning threshold.

Finally, the last term of Equation 3.1 indicates the costs,  $C(g)$ , to the incumbent associated with allocating  $g$  to a given county. These capture the opportunity costs associated with distributing  $g$  among the different counties in a given state so as to remain within the budget constraint. Costs are sufficiently convex such that  $\frac{\partial^2 \Pi_P}{\partial g^2} < 0$ .

A rational incumbent politician thus maximizes her expected pay-off over the allocation of state grants. That allocation is strongly affected by the share of newly legalized migrants,  $\alpha$ , in a county. From this follows Predictions 1 to 3 which help guide our empirical analysis.

**Prediction 1:** The optimal allocation of state aid increases in the share of newly documented migrants in a county.

**Prediction 2:** The optimal allocation of state aid is larger when  $P$  is eligible for re-election and less when (s)he is a lame duck.

**Prediction 3:** The optimal allocation of state aid becomes larger the closer the  $P$ 's expected vote share is to the winning threshold.

Proofs can be found in the appendix.

### 3.4 Data and Institutional Context

#### 3.4.1 Data

The key explanatory variable in our study is a measure of the number of IRCA applicants per 1,000 county inhabitants in the United States for the period between 1980 and 2000. In the treated counties (i.e. those counties that received at least 1 application for legal status), this value ranged from as little as .04 to as many as 50 applications per 1,000 county inhabitants.<sup>7</sup> To carry out our analysis, we compiled a new dataset from a number of different administrative sources. Table 3.1 shows summary statistics of

<sup>7</sup> By 1992, treated counties received, on average, eight applications per 1,000 county inhabitants which translates into some 2,800 applications for legal status per treated county.

the main variables in our study according to whether they are in treated or non-treated counties.

Our measure of IRCA applications per county comes from Baker (2015) who, in turn, takes it from the Immigration and Naturalization Service (INS). We also take from Baker (2015) measures of county poverty, population, unemployment and income, all of which are used as control variables in our analysis.

We aim at understanding the impact of the IRCA on the distribution of state and local finances and the sensitivity of this impact to political constraints. We thus add data on state and local finances taken from the US Census of Governments and use per capita inter-governmental revenues from state governments to local governments (counties, cities, municipalities) aggregated to the county as our dependent variable. We also utilize a host of governor related data including party affiliation of the governor, his or her name, an indicator for whether or not a governor is a lame duck and an indicator for whether (s)he enjoys line-item veto power in order to better understand the responsiveness of state politicians to the IRCA. This data is obtained from the Codebook for State Elections. We apply an instrumental variables strategy to confirm our OLS estimates, using the share of a county's 1960 population that is foreign-born as an instrument for the number of documented migrants per county post-1986. This variable is taken from the County and City Data Book prepared by the US Department of Commerce and the Census Bureau and made available by ICSPR under Study No. 7736.

### 3.4.2 Inter-Governmental Revenue and the Budget-Making Process

#### Inter-governmental revenue (IGR)

The primary dependent variable is per capita inter-governmental revenue (IGR) received by local governments (counties, cities, municipalities, aggregated to the county) from state governments.<sup>8</sup> The Census Government Finance and Employment Classification Manual defines this variable as “[a]mounts received directly from the state government, including federal aid passed through the state government and state aid channeled through intermediate local government (e.g counties) which have no discretion as to its distribution. [It] includes state grants-in-aid, regardless of basis of distribution.” Correspondence with staff at the Census Bureau confirms that “each state determines what specific funding sources (if any) are used for grants to local governments.” and that “each state determines the nature, amount and distribution of

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<sup>8</sup> We use inter-governmental revenue, state aid and state transfers interchangeably.

Table 3.1: Balance table: Treated v. untreated counties in 1984

	Treated			Untreated			Difference	
	Mean	S.D	Counties	Mean	S.D	Counties	Mean	S.E
<i>County Characteristics:</i>								
Transfers (per capita USD1999)	144.0	[150.4]	307	147.4	[344.9]	1886	-3.37	(20.0)
Log of Transfers (per capita USD1999)	4.33	[1.31]	307	4.30	[1.31]	1886	0.027	(0.081)
Unemployment Rate	7.98	[1.84]	328	7.98	[2.27]	1892	-0.0017	(0.13)
Poverty Rate	11.9	[5.85]	328	16.8	[7.48]	1892	-4.94***	(0.43)
Population (1000)	377.3	[607.6]	307	31.7	[29.8]	1886	345.5***	(14.1)
Log of County Income	9.53	[0.19]	328	9.29	[0.18]	1892	0.24***	(0.011)
County Tax Revenue (Pc)	133.9	[116.1]	307	129.1	[130.3]	1838	4.81	(7.91)
Log of Total County Crimes (Pc)	-3.36	[0.64]	328	-3.67	[0.73]	1892	0.31***	(0.043)
1960 Population Foreign Born (%)	4.62	[3.79]	328	1.77	[2.21]	1892	2.85***	(0.15)
<i>Governor Characteristics:</i>								
Lame-Duck Governor	0.32	[0.47]	328	0.39	[0.49]	1892	-0.071**	(0.029)
State Has Term Limits	0.50	[0.50]	328	0.56	[0.50]	1892	-0.065**	(0.030)
Share Democratic Governor	0.66	[0.48]	328	0.78	[0.41]	1892	-0.13***	(0.025)
Governor Re-elected	0.43	[0.50]	30	0.65	[0.48]	209	-0.21**	(0.094)
Percent Votes Cast For D-President	38.2	[9.62]	328	36.7	[10.3]	1892	1.52**	(0.61)

Notes: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

state grants internally.”<sup>9</sup>

Local governments have few major sources of local revenue, most notably property taxes and, to a much lesser extent, sales taxes. Inter-governmental revenue, therefore, is a type of budget support for local governments which comprises, on average, 30 percent of all local government revenue as shown in Figure C.2. Importantly, the local governments that receive this aid have “no discretion as to its distribution”. State and federal governments decide to what areas these revenues are directed. For example, state aid in education is intended to “support of local schools” but excludes “State grants for libraries”. The variable at our disposal is aggregate inter-governmental revenue at the county level and so it lumps together revenue intended for such areas as health, highways, education and public welfare.<sup>10</sup>

### **The budget-making process**

Our main contention is that state governors use their budgetary powers to allocate more resources to newly documented migrants in the hope of winning their future vote. A crucial question is thus how much power governors actually exert over the budget-making process. We take up this question in this section and demonstrate that, in fact, governors have substantial influence in the formulation and implementation of the states fiscal priorities.<sup>11</sup>

For the vast majority of states, the budget-making process takes an entire year: it begins sometime in July or August and for all but four states, the fiscal year begins on 1 July.<sup>12</sup> The state budget office is responsible for the analysis and preparation of the budget on behalf of the governor.<sup>13</sup> The budget-making process begins when the state budget office requests proposals from, and provides guidance to, various state-level agencies. This guidance typically includes state spending targets, assumptions for inflation and priorities of the governor. In the fall, the various agencies submit their

<sup>9</sup> Personal correspondence with Michael Fredericks of the Local Government Finance Statistics Branch of the Census Bureau on 26 November 2018.

<sup>10</sup> Although we only observe aggregate revenue, Table C.2 in the appendix details what is and what is not included in the inter-governmental revenue received from the state and gives an indication as to what types of local activity these revenues support.

<sup>11</sup> The information in this section draws from the National Association of State Budget Officers report on the budget-making process NASBO (2015).

<sup>12</sup> In theory, 30 states operate an annual budget cycle and 20 operate a biennial budget cycle. In practice, however, most states employ a combination of both: in those states that operate a yearly budget, it is not uncommon for the governor to release spending recommendations for a two-year time horizon. States on a two-yearly budget cycle, by contrast, often prepare a supplemental budget which, in many cases, acts as a de facto yearly budget.

<sup>13</sup> The budget director is appointed directly by the governor in 34 states; in 13 states he or she is appointed by the department head and in one state the governor and department head share responsibility for this appointment.

budget proposals to the governor who reviews them and provides additional direction. Once the governor's recommendations are incorporated, he or she presents the proposed budget to the state legislature in the winter season. After the legislature passes the budget, it requires the governor's signature to become law.

Importantly, governors enjoy a number of powers over the budget-making process, including being able to spend unanticipated funds without legislative approval or to withhold appropriations from agencies within the executive, legislative or even judicial branches of government. Crucially, governors enjoy various forms of veto authority over the state budget. Depending on the state, governors have the authority to either veto the entire budget or specific line-items of it, a power which gives them great leverage over the prioritization of the budget. Later in this chapter, we document heterogeneity in our results depending on the extent of veto power a governor enjoys.

### 3.5 Immigrant Legalization and Inter-Governmental Revenue

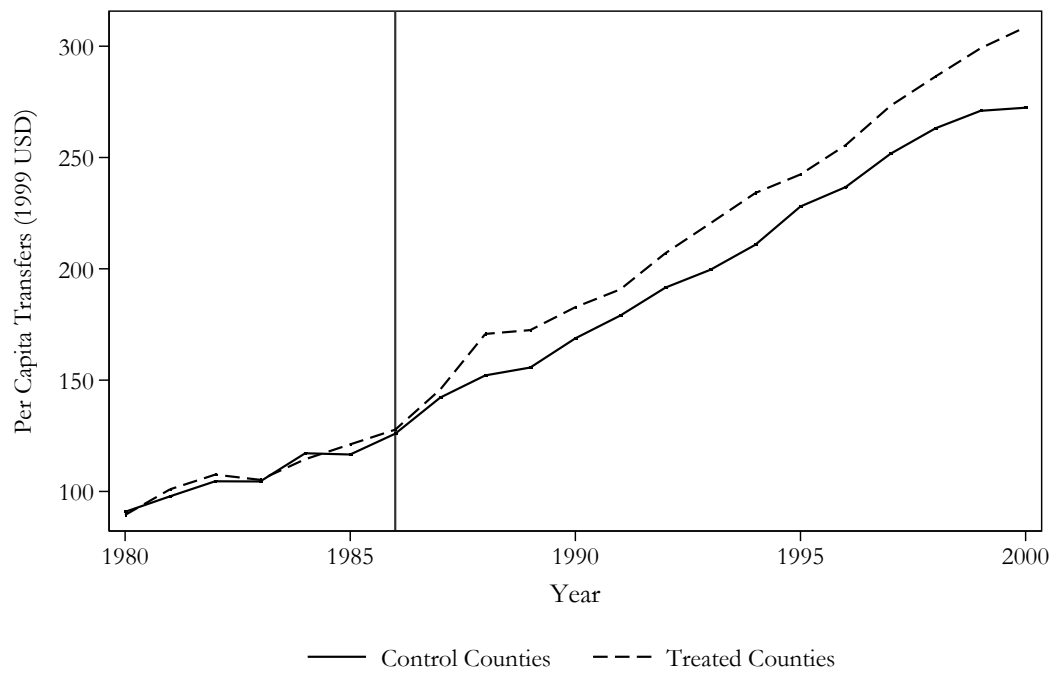
#### 3.5.1 The Evolution of IGR: Raw Data

Our aim is to understand the impact of documenting undocumented migrants on the distribution of inter-governmental transfers from state to local governments. The primary identifying assumption of our econometric model is that no other shocks occurred around the same time as the passage of the IRCA that correlate either with the number of legalized migrants in a given county or with the amount of inter-governmental revenue it received from the state. Prior to estimating the parameters of the model, therefore, it is informative to understand the evolution of IGR over time so as to lend credence to our identifying assumption. Figure 3.5 shows the trends in IGR as it appears in the raw data for the period between 1980 to 2000 in those counties that received applications for legal status with those that did not. As shown, the two county types developed along similar paths prior to the passage of the IRCA in 1986 and only after the passage of the law does one observe an appreciable difference between the two.<sup>14</sup>

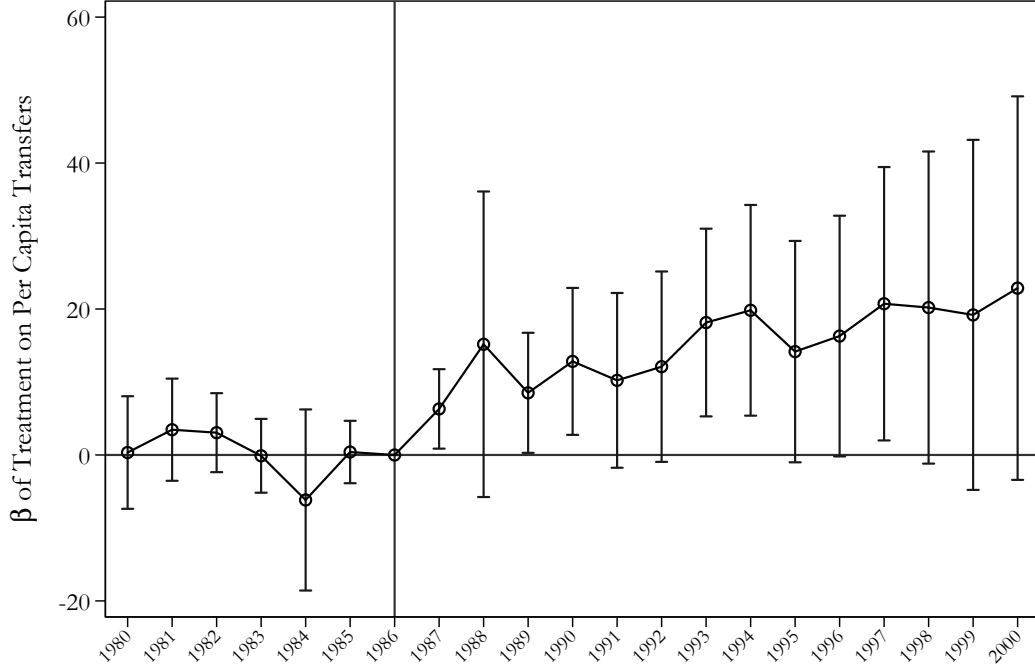
As a more rigorous test for pre-treatment differences, we plot the coefficients of

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<sup>14</sup>In Figure C.4 we show the evolution of a number of other county covariates that make clear that the IRCA is not associated with corresponding shocks in such county characteristics as population, income, tax revenue or public school enrollment. This is because undocumented migrants were (and still are) included in population estimates, are eligible (especially their children) for basic public services such as health and education and, to some extent, pay tax as some undocumented migrants obtain illegal social security documents.

**Figure 3.5:** Evolution of IGR

Notes: This graph compares per capita inter-governmental revenue received in those counties that never received applications for legal status (control) with those counties that did receive applications for legal status (treated) through the IRCA.

**Figure 3.6:** Treatment effect interacted with year dummies

Notes: This graph plots the regression coefficient on a treatment indicator when it is interacted with year dummies as specified in Equation 3.2. The regression only includes county fixed effects. The outcome, like Figure 3.5, is per capita inter-governmental revenue (in USD1999). Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent.  $N = 43,868$ .

an event study as specified in Equation 3.2:

$$y_{c,t} = \delta_c + \alpha_t + \sum_{j=1980}^{2000} \beta_j [T_c \times D_t^j] + \epsilon_{c,t} \quad (3.2)$$

Where  $y_{c,t}$  is per capita inter-governmental revenue from state to local governments (in 1999 USD) in county  $c$  in year  $t$ ;  $T_c$  is a binary variable set to one if a county received one or more applications for legal status post-1986 and zero otherwise; and  $D_t^j$  is a dummy set to one when  $t = j$  ( $\forall j \neq 1986$ ). We capture county fixed effects by  $\delta_c$  and time dummies by  $\alpha_t$  while  $\epsilon_{c,t}$  is an idiosyncratic disturbance term clustered at the county level. The results are shown in Figure 3.6, which indicate that the difference in transfers received between treated and non-treated counties shown in Figure 3.5 only becomes positive and significantly different to zero in the years after 1986, further increasing confidence in the reasonability of our identifying assumption.



### 3.5.2 Baseline Estimates

We impose more structure on model 3.2 in order to estimate the parameters of a difference-in-differences regression specification as detailed in Equation 3.3.

$$\ln(y)_{c,t} = \beta_0 + \delta_c + \alpha_t + \zeta_{st} + \beta_1 \cdot (T_c \times P_t) + \Theta \cdot \mathbf{X}_{c,t} + \epsilon_{c,t} \quad (3.3)$$

Where  $\ln(y)_{c,t}$  is the natural log of per capita inter-governmental revenue from state to local governments (in 1999 USD) in county  $c$  in year  $t$  and  $\delta_c$  and  $\alpha_t$  are defined as before. The treatment indicator,  $T_c$ , is now interacted with a binary variable  $P_t$ , that is one if  $t \geq 1986$  and zero otherwise. In addition, we include state-by-year fixed effects,  $\zeta_{st}$ , to account for state-specific time-varying shocks that might affect legalizations and transfers, including governor specific characteristics or other state-year-level political or economic shocks. We include a vector of county-level covariates,  $\mathbf{X}_{c,t}$ , that includes poverty and unemployment rates, income and population. As before,  $\epsilon_{c,t}$  is an idiosyncratic disturbance term clustered at the county level.<sup>15</sup>

The trends shown in the raw data are borne out in the regressions. Panel A of Table 3.2 shows our results across a number of variations of the model shown in Equation 3.3 and we see precisely estimated coefficients of similar magnitude across a number of specifications. In Panel B, we estimate the same parameters but using a measure of treatment intensity as specified in Equation 3.4. Here,  $\ln(IRCA+1)_{c,t}$  is the natural log of the cumulative number of IRCA applicants per 1000 county inhabitants (plus one) in county  $c$  in year  $t$ . The parameter of interest,  $\beta_1$ , can be interpreted as the elasticity of state transfers with respect to the cumulative number of per 1000 capita legalized applicants. All other parameters are defined as before.

$$\ln(y)_{c,t} = \beta_0 + \delta_c + \alpha_t + \zeta_{st} + \beta_1 \cdot \ln(IRCA + 1)_{c,t} + \Theta \cdot \mathbf{X}_{c,t} + \epsilon_{c,t} \quad (3.4)$$

Column (1) is our baseline estimate and suggests that counties affected by the IRCA received, on average, 7 percent more in per capita transfers than those that did not. Given that inter-governmental revenues make up, on average, 30 percent of local revenue, an increase in the order of 7 percent is significant. It corresponds to an increase of one and a half percentage points in the share of revenue received from inter-governmental sources. Using the measure of treatment intensity, the coefficient implies that a 1 percent increase in the number of per capita legalizations in a county is

<sup>15</sup>Because our unit of observation is the county and our treatment varies at this level, we cluster standard errors at the county. The results, however, are robust to clustering at higher levels, most notably the state. These results are captured in Table C.3 in the appendix.

**Table 3.2:** Inter-governmental revenue on IRCA legalizations

Log of Inter-governmental Revenue (per capita)					
	(1)	(2)	(3)	(4)	(5)
Baseline	Drop	Top 5	Pop < 409, 490	Matching	Linear Trends
<i>Panel A. Treatment Indicator</i>					
Treatment $\times$ Post	0.0709*** (0.0183)	0.0725*** (0.0199)	0.0493** (0.0193)	0.138*** (0.0314)	0.0556** (0.0241)
<i>Panel B. Treatment Intensity</i>					
Log legalizations	0.0610*** (0.0143)	0.0929*** (0.0217)	0.0448*** (0.0158)	0.0688*** (0.0157)	0.0462*** (0.0173)
Control Variables	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	No	Yes
County-Year Linear Trends	No	No	No	No	Yes
Observations	46,820	43,952	45,132	12,042	46,820
Number of Counties	2,686	2,526	2,612	604	2,686

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. Panel A shows results when using a treatment indicator and Panel B shows results when using a measure of treatment intensity which is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

associated with an increase in per capita transfers of about 0.06 percent. Within treated counties, moving from the 25<sup>th</sup> to 50<sup>th</sup> percentile of treatment intensity represents a 132 percent increase in per capita legalizations which translates to an increase of about 7 percent in per capita inter-governmental revenue.<sup>16</sup> Because treatment intensity is a much more precise measure of treatment, Equation 3.4 is our preferred specification and henceforth we will use the cumulative number of per 1000 capita IRCA applicants (plus one) as our main explanatory variable.

To ensure that our results are not driven by confounding factors, we undertake a number of sensitivity checks. We begin by re-estimating the parameters of the model in a sample that omits the five most treated states which, in per capita terms, correspond to California, Arizona, Florida, New Jersey and Nevada.<sup>17</sup> As shown in column (2), the results not only hold but become slightly larger, suggesting that legal status has a greater impact on the distribution of state finances in those states that do not absorb many newly legalized migrants. To alleviate concerns that the results are driven by very populous cities or counties—some of which may serve as so-called ‘sanctuary cities’—we rerun the regression, in column (3), on a sample restricted only to those counties with populations less than average county population size (i.e. 409,490) and obtain precisely estimated results, albeit of slightly smaller magnitude suggesting that the effect is strongest in more populated counties.

As shown in Figure 3.3, there are some 330 counties that received applications for legal status and just over 2,000 that did not. One might wonder, therefore, how comparable these two groups of counties are. To address these concerns, we use propensity score matching to generate a more comparable control group. That is, for every county in the sample, we generate, on the basis of its observable characteristics, a propensity score that indicates a given county’s likelihood to be treated. Then, for every treated county, we match the nearest neighbor from the untreated counties to generate a more comparable control group. In column (4), we rerun the model in this matched sample and obtain results almost identical to those of the baseline.<sup>18</sup>

Finally, in column (5), we rerun the baseline specification, adding to it county specific linear time trends. The idea here is to capture any differential trends with

<sup>16</sup> A very similar increase is associated with moving from the 50<sup>th</sup> to 75<sup>th</sup> percentile of treatment.

<sup>17</sup> Dropping the most treated states in terms of the absolute number of legalizations makes no difference to the results, nor does dropping the four states that border Mexico. These results are not reported.

<sup>18</sup> Changing the number of neighbors up to 5 does not change the result. Figure C.3 in the appendix shows the trends in inter-governmental revenue in treatment and control counties in the matched sample using nearest neighbor matching. The characteristics on which we generated the propensity score are county income, population, crime, tax revenue, poverty rate and unemployment in 1980. We drop state-year fixed effects to allow for the possibility that the best-matched control county for a given treated county may, in fact, lie in a different state. Matching within a state and leaving state-year fixed effects in the estimation does not change the results.

respect to the outcome variable that might arise over time for each county, trends which might render our identifying assumption implausible. This is the most demanding specification. That the result holds suggests that the relationship between immigrant legalization and the distribution of state aid is a robust one.

### 3.5.3 Robustness Checks

In Table 3.3 we carry out a number of further empirical checks to test the strength of the relationship. In column (1) we run a first-differences estimation using only two years in the sample: 1982 and 1992. The idea here is to skip intervening years to overcome issues with respect to timing of various sorts: different electoral cycles in different states, different budget response times and different IRCA application processing times. As shown, the legalization variable maintains its predictive power over per capita inter-governmental revenue.<sup>19</sup> In column (2), we use a county's 1980 population to carry out all per capita calculations as another way of ensuring population changes are not driving the results. To understand whether the relationship between immigrant legalization and the distribution of state aid is linear or quadratic, we include a quadratic term of the key explanatory variable in column (3). As shown, the linear variable retains its precision whereas the quadratic term enters imprecisely. In column (4) we include quadratic year trends and in column (5) we include additional county demographic controls, including the share of the population that is over 18 year of age, the share of the population that is Hispanic and the share of county households with children. Column (6) presents results from an instrumental variables estimation which will be explained in more detail in subsection 3.5.4.

In Table C.4, we replicate the baseline estimates using a *log-linear* specification to demonstrate that the results are insensitive to the logarithmic transformation of the data. We choose a *log-log* specification because (a) the legalization variable is unevenly distributed and (b) an elasticity is easier for interpretation.

### 3.5.4 Instrumental Variables

As a final empirical test to rule out endogeneity arising out of geographic factors associated with where the undocumented migrants settle, we use the share of a county's foreign-born population in 1960 as an instrument for the number of IRCA applicants post-1986. In doing so, we follow a number of other studies (Hildebrandt et al., 2005; Woodruff and Zenteno, 2007; McKenzie and Rapoport, 2010) that utilize historical rates of migration as an instrument for present levels.

<sup>19</sup> Figure C.5 plots the coefficients from a number of such regressions, each using a different time period for the difference estimation.

**Table 3.3:** Robustness checks

	Log of Inter-governmental Revenue (per capita)					
	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta y_{1992-1982}$	1980 PC	IRCA <sup>2</sup>	Year <sup>2</sup>	Add. Controls	IV
Log legalizations	0.0777*** (0.0201)	0.0579*** (0.0140)	0.102*** (0.0363)	0.0463*** (0.0179)	0.102*** (0.0177)	0.199*** (0.0635)
Log legalizations <sup>2</sup>			-0.0140 (0.0108)			
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	No	No	No	No	Yes	No
Linear Year Trends	No	No	No	Yes	No	No
Quadratic Year Trends	No	No	No	Yes	No	No
Observations	4,208	41,349	46,820	46,820	6,464	46,810
Number of Counties	2,104	2,211	2,686	2,686	2,407	2,685

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. **Log legalizations** is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). Column (2) carries out the analysis using per capita legalization and per capita transfers calculated with 1980 county population in the denominator. Additional controls in column (5) include the share of county population that is over 18, the share of county population that is Hispanic and the share of county households with children, which are only available for 1980, 1990 and 2000. Column (6) uses the share of foreign-born people in a county in 1960 interacted with year dummies as an instrument for log legalizations. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Because the instrument in question is time-invariant we interact the 1960 foreign-born variable with year dummies in order to take advantage of the panel structure of our data. Doing so produces results that are positive and precisely estimated, though an order of magnitude larger than the baseline as shown in column (6) of Table 3.3.<sup>20</sup>

### 3.5.5 Population Considerations

One may wonder whether our results are simply explained by a mechanical effect of having more people in the population eligible for social programs. We rule out this possibility for three reasons.

First, while the IRCA legalized approximately 3 million people in some three years, it did not lead to a corresponding increase in the population. This is because estimates of the undocumented population are obtained from a residual of two other population measures: (1) the total foreign-born population (obtained through the Census) and (2) the legally resident population (known by the INS). The undocumented population estimate is the residual when (2) is subtracted from (1); hence population estimates undertaken by the Census Bureau are inclusive of undocumented migrants (Baker and Rytina, 2013). This fact is made evident in Figure C.4 which shows population growth in treated and untreated counties. As illustrated, neither type of county experienced appreciable growth in population in the years before or after the passage of the IRCA. Accordingly, even if funds were transferred by formula on the basis of a county's population, the fact that there is no population growth associated with the IRCA alleviates our concern that mechanical population forces drive our results.

Second, a feature of the IRCA was that it “barred” the newly legalized “from participation in programs of financial assistance furnished under federal law on the basis of financial need for a period of five years from the effective date of each alien's lawful temporary resident status” (DHHS, 1991). Moreover, given the demographic characteristics of the newly legalized discussed previously and that the children of undocumented migrants were already eligible for public services such as schooling pre-IRCA, we find it unlikely that our results are explained by mechanical increases as a

<sup>20</sup> Another option is to run 13 cross-section regressions for each year between 1988 and 2000 where each variable is differenced from its 1982 value, as shown in specification 3.5, and the differenced variable is then instrumented in the cross-section. The regression coefficients are plotted in Figure C.6 and confirm, both in terms of precision and the timing of the effect, the baseline estimates shown in Figure C.5.

$$\ln(y)_{c,t-1982} = \beta_0 + \beta_t \cdot \ln(IRCA + 1)_{c,t-1982} + \Theta_t \cdot X_{c,t-1982} + \epsilon_{c,t} \quad (3.5)$$

result of social assistance eligibility criteria being satisfied at the state level.<sup>21</sup>

Third, the dependent variable used throughout our study is a measure of *per capita* transfers from state to local governments. If the policy was simply associated with a mechanical increase in transfers, we might expect the overall *level* of transfers to increase but there would be no reason, ex-ante, to expect any change in the amount of *per capita* transfers. That per capita transfers are a function of the number of legalizations in a county seems to suggest that the transfer activity we observe is more than a mechanical increase that might arise out of a transfer formula based on population considerations.

### 3.5.6 SUTVA

The stable unit treatment value assumption (SUTVA) holds that the potential outcome of a unit of observation is unaffected by the treatment status of other units. In this particular context, therefore, a question arises as to whether counties affected by the IRCA receive their transfers at the expense of those counties not affected by the law or whether these funds come from other sources. To better understand the nature of the treatment effect, and to understand whether SUTVA holds in this particular setting, we undertake two exercises.

First, there are four states in the sample that were unaffected by the IRCA. These are North and South Dakota, Vermont and Wyoming. As a first step, therefore, we run the baseline specification using the treated counties from treated states and only the control counties from these four control states. The idea here is that if the result is reflective of a distributive politics channel where the governor takes from control counties in order to give to treated counties, we should see a smaller effect when we compare treatment and control counties from different states. To compare counties across state borders, we drop state-year fixed effects (and include year fixed effects instead) and generate the results presented in Table 3.4. They indicate that, by and large, the treatment effect is not coming at the expense of control counties.

To probe this question further, we turn to state revenue data from the Census of Governments. Here we observe a state's revenues from various tax sources as well as from the federal government by way of inter-governmental revenue from the federal government to the state. The coefficient on per capita legalizations at the state level shown in Table 3.5 indicates that revenue from the state increases as a function of

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<sup>21</sup>Later in the paper, we utilize Census of Government expenditure data to better understand the impact of legal status on various categories of local expenditure and find that the IRCA does not have a significant impact on local welfare expenditure (as shown in Figure 3.8).

**Table 3.4:** IRCA and SUTVA

	Log of Inter-governmental Revenue (per capita)	
	(1) Full Sample	(2) Control States
Log legalizations	0.0988*** (0.0143)	0.0782*** (0.0177)
Control Variables	Yes	Yes
Year Fixed Effects	Yes	Yes
County Fixed Effects	Yes	Yes
Observations	46,826	10,771
Number of Counties	2,686	749

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. **Log legalizations** is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). In column (1) we exploit the full sample. In column (2) we use only treated counties from treated states and the control counties from the four control states in the sample. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$



**Table 3.5:** State revenues and IRCA

	Log of State Revenue From...		
	(1) Sales Tax	(2) Income Tax	(3) Federal Gov't
Log Legalizations, State	0.029** (0.013)	0.022* (0.011)	0.013* (0.007)
Control Variables	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Observations	853	853	916
Number of States	41	41	44

Notes: The dependent variable is the log of state revenue from various sources. **Log legalizations, State** is the log of the cumulative number of IRCA applications in a given state in a given year per 1000 state inhabitants (plus one). We control for state unemployment, population and income. Standard errors (shown in parentheses) are clustered at the state level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

IRCA documented migrants in a state.<sup>22</sup>

### 3.6 Political Economy Mechanisms

The analysis in the preceding section demonstrated that the relationship between immigrant legalization and the distribution of state finances is a robust one. In this section, we investigate to what extent the relationship is reflective of discretionary, political choices made by state governors and to what extent it is merely reflective of mechanical, bureaucratic forces that oblige the governor to better service the areas where the documented migrants reside. To shed light on these issues, and on the mechanisms that drive the result, we turn our attention to the political constraints on, and the political context of, the state governor. The contention here is simple. If, on the one hand, the increases in per capita transfers associated with the IRCA are the result of mechanical forces, the results ought to be entirely insensitive to political context or constraints. If, on the other hand, the transfers are the result of discretionary choices made by state governors in an effort to bolster political support, then it is not unreasonable to expect state aid to display some sensitivity to political context.

<sup>22</sup>To alleviate concerns that the result is driven by increased inter-governmental revenue from the federal government which simply passing through the state, the Census Bureau explains that “federal aid that is given to the state to then be distributed to local governments is normally considered state aid because states usually have discretion over the distribution.”

### 3.6.1 Political Party Heterogeneity

We begin by investigating the sensitivity of our results to the party affiliation of the governor. Column (1) of Table 3.6 indicates that the per capita transfers a county receives in response to the IRCA policy are positive and significant and that this amount increases by about half when the governor is a Democrat as compared to when he or she is a Republican.<sup>23</sup> In column (2) we test whether state governors give more to counties that are politically aligned with them, in the sense that a given county's political leaning (measured by its Presidential election results) align with those of the party of the governor.<sup>24</sup> As shown, state aid increases to a county affected by the IRCA regardless of whether the county's political leaning is aligned with that of the governor. Accordingly, these results confirm that the distributional impact of the IRCA is driven more by political factors at the state level.<sup>25</sup>

### 3.6.2 Term Limits and Election Cycles

Next, because our data includes the names of state governors, we are able to compare state-to-county transfers under a single governor over time as he or she faces different political constraints and election cycles. By way of example, we consider the transfers in just one state, Georgia, over the political career of one of its governors, Zell Miller (D), from 14 January 1991 to 11 January 1999. Governor Miller served two terms in office: from 1990 to 1994 and from 1994 to 1998. Georgia has a two-term limit constraint on the Governor.<sup>26</sup> Therefore Zell Miller was eligible for re-election in his first term but he was a lame duck in his second. Georgia comprises 159 counties of which we have data for 137. From among the counties for which we have data, eight received legalized migrants as a result of the IRCA and 129 did not. Figure 3.7 shows the trends in transfers during Zell Miller's tenure as Governor. As shown, the counties that received no legalizations experienced a steady decline in the amount of per capita transfers received. The eight counties that received legalizations, by contrast, exhibit

<sup>23</sup>Figure C.7 and Table C.5 show, perhaps unsurprisingly, that the Democratic vote share in Presidential elections increases as the share of IRCA migrants in a county increases.

<sup>24</sup>We use Presidential election data as a proxy for Gubernatorial electoral returns because the Gubernatorial election data is available only as of 1990, after the variation in legalizations has ended. A county's Presidential election outcomes do follow its Gubernatorial outcomes quite well as shown in Figure C.8.

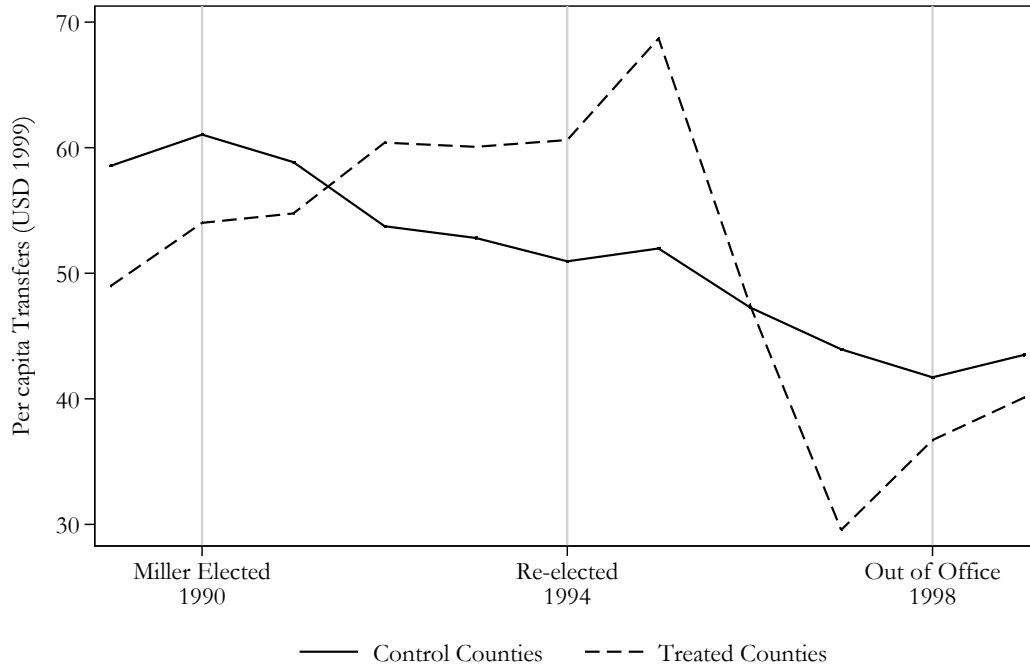
<sup>25</sup>In Table C.6 we explore whether the partisan composition of Congress has any explanatory power over the results. To this purpose, we interact the legalization variable with indicators for whether a given state's Members of Congress or Senators were majority Democrats or not. As shown, the party affiliation of a state's federal representatives has no explanatory power on the overall manner in which the state budget is distributed in response to IRCA.

<sup>26</sup>In the United States 26 states had term limits from 1980 to 1986, the majority of which were limited to 2 terms. Thereafter, the number of states with term limits increased to over 30, again the vast majority with a 2 term limit.

**Table 3.6:** Legalization and political heterogeneity

	Log of Inter-governmental Revenue (per capita)			
	(1) Party	(2) Aligned	(3) Incentive	(4) Election Cycle
Log legalizations	0.0516*** (0.0160)	0.0544*** (0.0143)	0.0420*** (0.0143)	0.0527*** (0.0157)
D-Governor $\times$ Log legalizations	0.0234* (0.0121)			
Aligned		-0.000185 (0.00754)		
Aligned $\times$ Log legalizations		0.0139 (0.00936)		
Log legalizations $\times$ Incentive			0.0246** (0.0107)	
Log legalizations $\times$ Election Year				0.0188*** (0.00706)
County Controls	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	46,393	46,820	41,807	30,694
Number of Counties	2,686	2,686	2,677	2,381

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. **Log legalizations** is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). **D-Governor** is an indicator that is 1 if the party of the governor is Democratic and 0 if Republican. **Aligned** is an indicator that is 1 if the county's election results in the most recent Presidential election (Democrat or Republican) are aligned with the party of the Governor and 0 if not. **Incentive** is an indicator that is 1 if a governor is not a lame duck and 0 otherwise. **Election Year** is an indicator according to whether a governor, who is no lame duck, is in an election year or not. The baseline effects of **D-Governor**, **Incentive** and **Election Year** are captured by state-year fixed effects and are thus unable to be estimated. The outcome variable in column (4) is lagged by one year. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Figure 3.7:** Transfers in Zell Miller's Georgia

Notes: This graph plots inter-governmental transfers in 1999 USD in Georgia during Governor Zell Miller's time in office. Control counties are those that never received applications for legal status through the IRCA whereas treated counties did receive applications for legal status through the IRCA. Georgia comprises 157 counties of which we have data on 137: eight affected by the IRCA and 129 were not.

a great deal of variation. In Governor Miller's first term, transfers to these counties increase only to drop off drastically in his second term when he is no longer eligible for re-election. The question that arises, therefore, is to what extent inter-governmental revenue differs as governors face term limits and how much of this difference is driven by the IRCA policy. Similarly, one wonders to what degree state aid fluctuates in the face of gubernatorial election cycles.

The regressions in Table 3.6 investigate these questions. In column (3), we interact the legalization variable with a binary variable that is one when the governor is eligible for re-election and zero when (s)he is a lame duck owing to a term limit and the result indicates that the difference in transfers between lame duck and non-lame-

duck governors is indeed positive and significant.<sup>27</sup> We carry out a similar analysis in column (4), this time analyzing sensitivity to the gubernatorial election cycle. Here, we lag the outcome variable by one year to better understand the dynamics of inter-governmental revenue in the year prior to an election. The result suggests that counties affected by the IRCA receive about 35 percent more in inter-governmental revenue in the year prior to a gubernatorial election.

### 3.6.3 Electoral Competition

To shed further light on mechanisms, we examine the sensitivity of transfers to electoral competition. The logic is similar to those of term limits. If the transfers we observe are discretionary, we would expect more resources to flow into those counties whose previous electoral races have been more competitive. To test this hypothesis, we generate the absolute value of the winning margin between Democrats and Republicans in the 1984 and 1988 Presidential election and identify those counties whose win margins are tighter than the tightest 25, 10 and 5 percent of the distribution in both elections. We then interact the legalization variable with an indicator for whether a given county is competitive and compare this interaction across two time periods: 1984 to 1988 when the IRCA migrants were ineligible to vote and 1988 to 1992 when governors fight for the votes of the newly legalized. Results are shown in Table 3.7 and indicate that the impact of legalization on state-to-county transfers is amplified in the post-1988 period when a given county is more politically contested.

### 3.6.4 Veto Power and State Legislatures

As mentioned earlier in the paper, governors exercise strong influence over the budget-making process in a given state. In this subsection, we focus on arguably the most influential of these powers: the line-item veto.<sup>28</sup> This accords with a range of theoretical and empirical literature that documents the growing importance of the state executive branch relative to the legislative branch in setting state priorities in general (Clych and Lauth, 1991) and in shaping the state budget in particular (Kousser and Phillips, 2012;

<sup>27</sup> The advantage of this approach is that it allows us to trace the evolution of transfers over the course of a single governors term. However, one might be concerned that this specification does not allow us to estimate a governor's electoral incentive arising from the IRCA since the IRCA ceases to produce meaningful variation in the number of documented migrants after 1992. To address this, we re-estimate the parameter of interest, limiting the sample to the period only between 1989 and 1994 and compare governors who are lame ducks in this period with those who are not. Results are shown in Table C.7 and indicate that governors with an electoral incentive allocate significantly more resources than their lame duck counterparts as the share of documented migrants in a county increase.

<sup>28</sup> Figure C.9 illustrates how this power has grown stronger over time.

**Table 3.7:** Legalization and tightness-of-the-race

	Log of Inter-governmental Revenue (per capita)		
	(1) Tightest 25%	(2) Tightest 10%	(3) Tightest 5%
Log legalizations	0.103*** (0.0353)	0.0954*** (0.0324)	0.0929*** (0.0322)
Log legalizations $\times$ Tight25 $\times$ Post-1988	0.0698* (0.0364)		
Log legalizations $\times$ Tight10 $\times$ Post-1988		0.0736* (0.0377)	
Log legalizations $\times$ Tight5 $\times$ Post-1988			0.0688** (0.0350)
Control Variables	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes
Observations	18,585	18,585	18,585
Number of Counties	2,561	2,561	2,561

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. **Log legalizations** is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). **Tight25**, **Tight10** and **Tight5** indicate, respectively, whether the outcome of the 1984 and 1988 Presidential election in a given county was more competitive (defined as the absolute difference between votes for the Republican and Democratic candidate) than those in the 25<sup>th</sup>, 10<sup>th</sup> and 5<sup>th</sup> percentile of the competitiveness distribution. **Post-1988** is 1 for the period between 1988 and 1992 and 0 for the period from 1984 to 1988. The baseline effects of **Tight25**, **Tight10**, **Tight5** are captured by county fixed effects. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Barrilleaux and Berkman, 2003).

In columns (1) and (2) of Table 3.8, we split the sample according to whether or not the state governor enjoys line-item veto power and estimate the parameters of our baseline specification.<sup>29</sup> As shown, the legalization variable has strong predictive power over inter-governmental revenue in those states where the governor has line item veto power.

In column (3) of Table 3.8, we test the sensitivity of transfers to the relationship between the state governor and the state legislature. Although governors do enjoy increasing power over the state budget, legislatures still play a role. And if the result is discretionary, as opposed to mechanical, we might expect it to display important heterogeneities depending on the relationship between the executive and legislative branches of state government. Accordingly, we generate an indicator that is one when the party of the governor is aligned with the partisan majority of the state legislature and zero otherwise.<sup>30</sup> The result indicates that, although transfers increase as the share of IRCA applicants in a county increases when there is no political alignment between the governor and the legislature, the result increases by about 50 percent when there is political alignment, further underscoring the politically discretionary nature of these transfers.

### 3.6.5 Re-election Considerations

How might these political economy results impact a governors re-election chances? Our data includes an indicator for whether a particular governor was re-elected and we exploit this variable to understand whether the share of documented migrants in a state affects re-election chances in any way. Because this outcome varies at the state level over time, we can only include state and year fixed effects separately, denoted by  $\gamma_s$  and  $\alpha_t$  respectively, but not jointly. Moreover,  $Post_{1992}$  indicates the period in which the documented migrants likely earn the right to vote.<sup>31</sup> Our specification is thus expressed in equation 3.6, where  $R_{s,t}$  is an indicator for whether the governor in state  $s$  has been re-elected in year  $t$ .

$$R_{s,t} = \beta_0 + \gamma_s + \alpha_t + \beta_1 \cdot \ln(IRCA + 1)_{s,1992} \cdot Post_{1992} + \Theta \cdot \mathbf{X}_{s,t} + \epsilon_{s,t} \quad (3.6)$$

The result is presented in column (4) of Table 3.8. It suggests that as the share of documented migrants in a state increases, so too does the governors chances for

<sup>29</sup> Specifically, we compare states where the governor has line-item veto power to states where the governor has a simple veto, but not line-item veto.

<sup>30</sup> This includes when the state legislature is split or has a majority of the opposite party to the governor.

<sup>31</sup> Figure C.10 displays the trends in legalization and naturalization.

**Table 3.8:** Veto, state legislatures and re-election

	(1) Veto	(2) No Veto	(3) Alignment	(4) Re-elected
Log legalizations	0.0640*** (0.0131)	0.00709 (0.0301)	0.0535*** (0.0139)	
Log legalizations × Alignment			0.0245** (0.0109)	
Log legalizations <sub>1992</sub> × Post <sub>1992</sub>				0.217*** (0.0623)
County controls	Yes	Yes	Yes	No
State controls	No	No	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	No
State-Year Fixed Effects	Yes	Yes	Yes	No
State Fixed Effects	No	No	No	Yes
Observations	41449	5356	46820	181
Number of Counties	2,555	670	2,686	-
Number of States	-	-	-	43

Notes: In columns (1) to (3), the dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. In column (4), the outcome is a binary variable that is 1 if the governor is re-elected and 0 otherwise. **Log legalizations** is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one) and **Log legalizations<sub>1992</sub>** is the log of the cumulative number of IRCA applications in 1992 aggregated at the state level. Column (1) restricts the sample to those Governors who enjoy line-item veto power and column (2) restricts the sample to those Governors who enjoy no such power. **Alignment** is an indicator that is one when the party of the Governor is aligned with the partisan majority of the state legislature and 0 when it is not. The baseline effects of **Aligned** is captured by state-year fixed effects and thus cannot be estimated. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. State controls are the same, but aggregated to the state level for the specification in column (4) and also include the party of the governor. Standard errors (shown in parentheses) are clustered at the county level in columns (1) to (3) and at the state level in column (4). \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$



re-election after documented migrants are eligible to vote, lending further credence to the political economy nature of our baseline results.<sup>32</sup>

### 3.7 Capturing the Vote of the Newly Legalized

Thus far, we have demonstrated a robust relationship between immigrant legalization and the distribution of public resources. Governors allocate more resources to counties as the share of newly documented migrants in those counties increase, an allocation which is particularly sensitive to the political context of the incumbent. Until now, however, we have implicitly assumed that the inter-governmental revenue is intended to capture the vote of the migrants who obtained legal status through the IRCA. Of course, because the inter-governmental revenue we observe is aggregated at the county level, we are unable to verify with certainty which constituents these monies are actually intended to reach.<sup>33</sup> In this section, therefore, we present a number of pieces of evidence that demonstrate that it is indeed the IRCA migrants who motivate the state governor.

#### 3.7.1 Attitudes Towards Migrants

Like today, undocumented migration was a politically charged issue at the time of the IRCA. A notable opponent of the IRCA, and of undocumented migration more generally, was Governor Pete Wilson, Republican governor of California, who ran a campaign of fear and anti-migrant propaganda. In his 1994 re-election campaign, Governor Wilson pinned his hopes onto Proposition 187, the “Save Our State” ballot initiative, and the Republican Party offered ideological and financial backing to

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<sup>32</sup> One question that might arise is how visible these transfers are in the sense of their ability to influence public opinion about the incumbent. In this respect, an assumption in our conceptual framework is that voting decisions in a county are based on local economic conditions and that voters base their decision not just on local conditions immediately prior to the election but rather throughout the term of the governor. In the terminology of Elinder et al. (2015), voters in our framework base their decisions *retrospectively* (i.e. based on the implemented policies of the incumbent) rather than *prospectively* (i.e. based on the promises candidates make). In Table C.8 we investigate how the allocation on state aid affects local spending and the coefficient suggests that across all categories of local spending, the elasticity of such spending with respect to state aid is positive and precisely estimated, indicating that these transfers are visibly felt at the local level.

<sup>33</sup> Moreover, because this revenue is dedicated to such purposes as health and road improvements, the governor can use it to win over several constituents and not just a single one. Indeed, a key difference between the governor and his or her legislative counterparts is that the governor can shape a states fiscal priorities so as to build winning coalitions from among otherwise competing constituents; legislators on the other hand often vote over single issues, increasing the likelihood of generating ‘winners’ and ‘losers’ from any given vote (Cascio and Washington, 2014).

see the proposition go through.<sup>34</sup> Proposition 187 prohibited undocumented migrants from using non-emergency public services and required the providers of such services to immediately report undocumented migrants for deportation. It was passed by California's voters only to be struck down by a federal court. The proposition, and Wilson's campaign to support it, was highly controversial and left somewhat of an enduring legacy. Bowler et al. (2006), for example, find that racially charged ballot initiatives in California—and specifically Proposition 187—are significantly associated with a shift in political support away from the Republican party and towards the Democratic party on behalf of non-Hispanic white voters as well as Latino voters.

In light of this political context, it seems reasonable to ask to what degree our results are actually driven by governors catering to anti-migrant sentiment arising out of the IRCA rather than to the needs of the documented migrants themselves. We examine this question first by quantifying the impact of Governor Wilson's term in office and of Proposition 187 on state aid. In column (1) of Table 3.9, therefore, we restrict the sample to consider only California during the eight years for which Governor Wilson was in power (1991 to 1998) and exploit variation in county-level voting outcomes for Proposition 187. Forty-seven of California's 55 counties voted for the Proposition and eight rejected it and the results varied from as little as 29 percent in favor to as much as 77 percent. Perhaps unsurprisingly, counties more affected by the IRCA received less inter-governmental revenue during Governor Wilson's tenure. However, this result wiped away and made positive for counties with a vote share of 49.5 percent or higher. However, neither of the coefficients are precisely estimated, which suggests that the impact of immigrant legalization on state aid is not, in California at least, confounded by anti-migrant sentiment. In column (2) we estimate the parameters of the baseline specification excluding California, the state with the strongest expression of anti-migrant sentiment at the time and the results hold. In the years following proposition 187, ten other states passed ballot initiatives or laws similar to that of Proposition 187.<sup>35</sup> Dropping these states from the analysis—presumably the states where governors had the strongest incentives to cater to anti-migrant sentiment—does not alter the results in any meaningful way.

As a more general check, we merge the legalization variable with data from the General Social Survey (GSS), which includes a range of questions on attitudes

<sup>34</sup>In a dramatic re-election advertisement, Governor Wilson states "I'm suing to force the Federal Government to control the border and I'm working to deny state services to illegal immigrants. Enough is enough." (Transcribed from the Television Ad which can be found at: <https://www.youtube.com/watch?v=1LIzss2HHgY..> Accessed 8 March 2018.

<sup>35</sup>These are Arizona, Colorado, Florida, Georgia, Illinois, Nevada, New Mexico, New York, Oklahoma and Texas as reported by Richard Lacayo (December 19, 2004) in the following report: <https://ti.me/2PbD7YE>. Accessed 8 March 2018.

**Table 3.9:** IRCA and anti-migrant sentiment

	Log of Inter-governmental Revenue (per capita)		
	(1) Wilson	(2) No Cali	(3) No Anti-Migrant States
Log legalizations	-1.844 (1.601)	0.0834*** (0.0182)	0.0579** (0.0238)
Log legalizations $\times$ Prop 187 VS	0.0372 (0.0241)		
Control Variables	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes
Observations	428	45,690	32,232
Number of Counties	54	2,632	1,864

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. **Log legalizations** is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). **Prop187 VS** is the county vote share for Proposition 187. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Column (3) excludes the 10 states (plus California) that passed ballot initiatives or laws similar in spirit to those of Proposition 187. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table 3.10:** IRCA and attitudes towards migration (GSS survey)

	Attitudes on Undocumented Migrants			Attitudes on Documented Migrants	
	(1) Given Work Permits	(2) Work Hard	(3) Deported	(4) Increase Crime	(5) Take Jobs Away
Log legalizations	1.669** (0.718)	0.540*** (0.130)	-1.818* (0.815)	-3.673* (1.698)	-3.760* (1.858)
Control Variables	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	No	No	No
Region Fixed Effects	No	Yes	No	No	No
Observations	873	16,179	705	581	639
Number of Regions	9	9	9	9	9
Years in Sample	1994	1980 — 1998	1996	1996	1996

Notes: This table uses General Social Survey (GSS) data merged with the legalization data. Log legalizations is the log of the cumulative number of IRCA applications in a given region in a given year per 1000 region inhabitants (plus one). The outcome variables are all binary indicators on various attitudes towards documented and undocumented migrants. Control variables include individual income, employment status, marital status, age, educational attainment and race. Standard errors (shown in parentheses) are clustered at the region level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

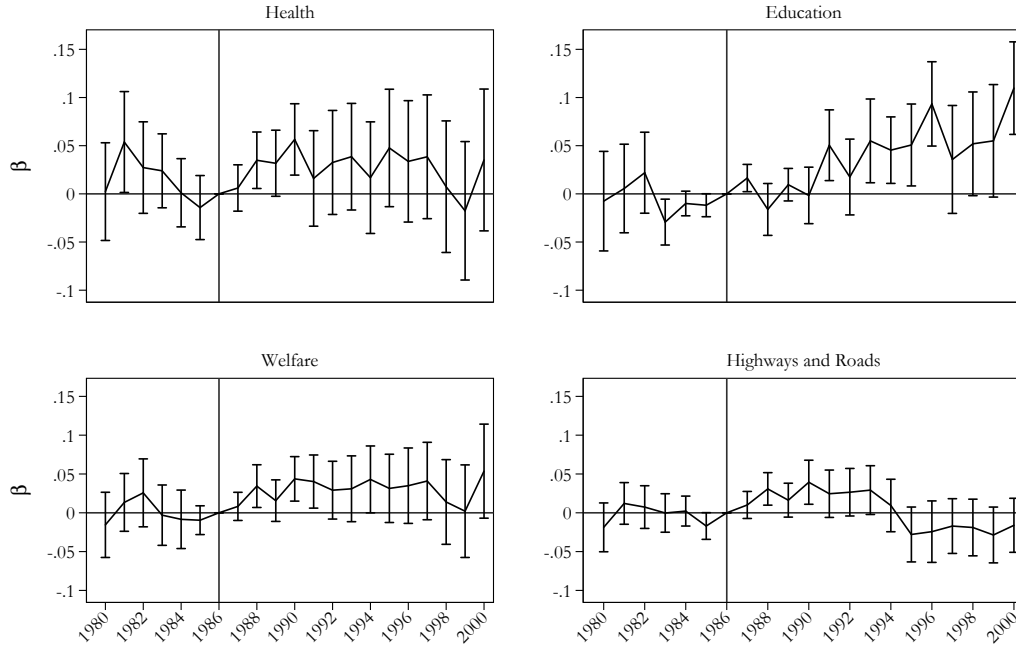
towards migration. As indicated in the various columns of Table 3.10, a higher share of legalized migrants in a region tends to be positively correlated with positive attitudes towards documented and undocumented migrants. Individuals residing in such regions tend to think undocumented migrants work hard, deserve work permits and ought to be protected against deportation. Such individuals are also of the opinion that documented migrants neither increase crime nor take jobs away from native citizens. Given these attitudes, and given the fact that the IRCA was not associated with an influx of migration but rather a change in the legal status of already resident migrants, we find it unlikely that state aid in IRCA-affected counties is intended to satisfy nativist sentiment or general opposition to the amnesty.

### 3.7.2 The IRCA, Local Expenditure and Hispanic Outcomes

Finally, we turn our attention from county revenue to county expenditure in an effort to better understand in which areas and, potentially, on which constituents county revenue is spent.<sup>36</sup> Figure 3.8 plots event study estimates when the legalization intensity in 1992 is interacted with year dummies in regressions with various categories of local expenditure as the outcome. These figures suggest that the IRCA led to increases in local expenditure in the areas of health, education and welfare but that these increases are estimated with precision for education expenditure beginning in 1991.

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<sup>36</sup> Figure C.11 displays various categories of local government expenditure as a share of total local expenditure.

**Figure 3.8:** Event study estimates of local expenditure on legalization

Notes: This graph plots the regression coefficient on the log of the cumulative number of IRCA applications in a given county per 1000 county inhabitants (plus one) in 1992 when it is interacted with year dummies. The outcome variables are the log of per capita county expenditure in health, education, welfare and highways and roads. The regressions control for poverty and unemployment rates, log of population and log of income, all aggregated to the county level as well as county and state-by-year fixed effects. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent.  $N = 34,840$  for all regressions.

To understand whether these educational expenditures were intended to benefit the newly documented migrants and/or their families, we calculate race-specific high school completion rates to test whether the counties that were affected by the IRCA also experienced improvements in Hispanic high school completion. To carry out this exercise, we obtain data from the 2010 decennial census in order to estimate the impact of the IRCA on an individual's educational outcomes. Rather than compare individuals in treated and non-treated counties before and after the passage of the IRCA, we now compare individuals in treated and non-treated counties in cohorts that entered middle school before the passage of the IRCA (and hence were less likely to benefit from additional educational expenditure) with those cohorts that entered middle school after the IRCA passed (and hence were more likely to benefit from additional funds). Accordingly, we construct 20 middle school entry cohorts from 1980 to 2000. An

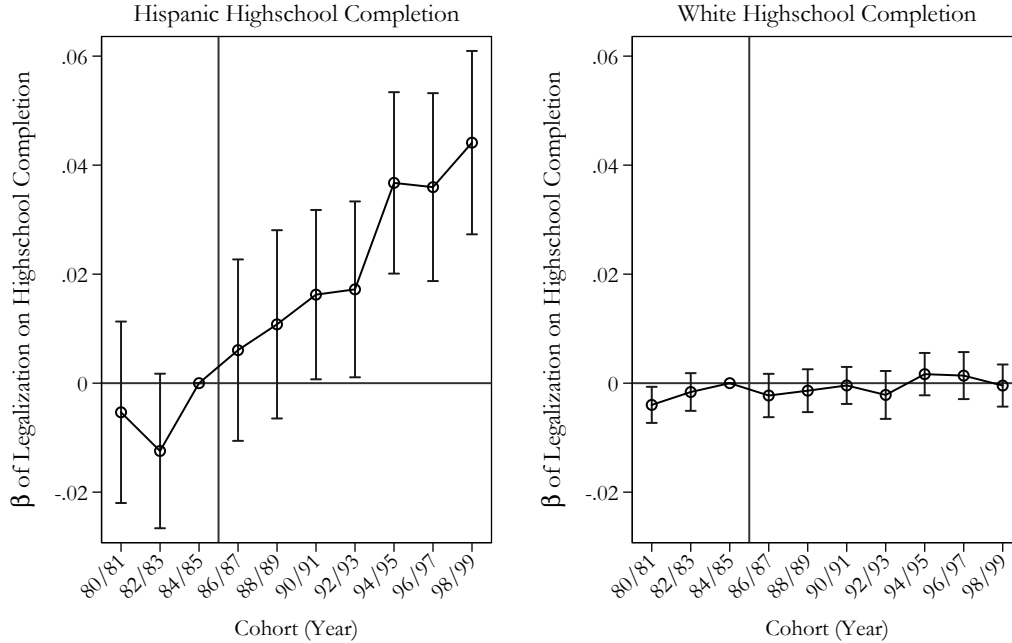
individual in the 2010 census is placed in a middle school entry cohort depending on which year he or she was 12 years of age. The specification is detailed in equation 3.7, where  $H_{i,c,mse}$  is an indicator if individual  $i$  in county  $c$  and in middle school entry cohort  $mse$  has 12 years of education or more. To get more precise estimates, we combine cohorts to pairs of two. County and middle school entry cohort fixed effects are captured by  $\delta_c$  and  $\psi_{mse}$ , respectively and  $D_{mse}^j$  is an indicator that is one when  $j = mse$  and zero otherwise  $\forall j \neq 7$ . All other terms are defined as before.

$$H_{i,c,mse} = \beta_0 + \delta_c + \psi_{mse} + \sum_{j=1}^{10} \beta_j [\ln(IRCA + 1)_{c,1992} \times D_{mse}^j] + \epsilon_{c,mse} \quad (3.7)$$

We first run the specification on a sample of only Hispanic individuals and then on a sample of only Caucasian persons and plot the corresponding coefficient,  $\beta_j$ , as shown in Figure 3.9. This coefficient estimates the change in the slope of high school completion between individuals in high and low treatment intensity counties across various middle school entry cohorts. The event study estimates indicate that for Hispanic persons, residing in a county affected by the IRCA led to a positive and significant impact on that person's likelihood of completing high school, provided they entered middle school after 1990. Indeed, there is no distinguishable difference in the likelihood of completing high school between individuals in high-treated and low-treated counties if they began middle school prior to this time. The timing of this effect suggests that the increased high school completion rates arise not just from legal status but from additional resources that these counties receive for education. For Caucasians individuals, by contrast, residing in an IRCA-affected county has no distinguishable impact on high school completion probability, regardless of when they began middle school. These results lend further credence to our hypothesis that state politicians targeted newly documented migrants.<sup>37</sup>

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<sup>37</sup>In the appendix, we also try to understand the electoral relevance of the IRCA. To address this, we obtain individual-level data from the November voter supplement of the CPS which includes indicators for whether a person voted in the November election. Our aim is to understand how individual voting is affected by both the IRCA and inter-governmental revenue. We thus plot the marginal effect of the IRCA on the propensity of an individual to vote along the distribution of inter-governmental revenue as shown in Figure C.12. Importantly, though, we have data for only three periods: 1996, 1998 and 2000 because the CPS does not include county identifiers for earlier time periods. As such, these results include no pre-treatment observations and ought to be taken as suggestive. Table C.9 presents the results of the margins plot in table form along with other results from the CPS.

**Figure 3.9:** Event study estimates of high school completion on legalization

Notes: This graph plots the regression coefficient on legalization intensity in 1992 when it is interacted with middle school entry cohort dummies as shown in equation 3.7. A person is placed in a middle school entry cohort depending on the year in which they were 12 years of age. The outcome variable, taken from the 2010 decennial census, is an indicator that is one if an individual in a given county and middle school entry cohort completed high school or more and zero otherwise. The regressions include county and cohort fixed effects. The panel on the left plots coefficients when the sample is restricted only to Hispanic individuals whereas the figure on the right estimates the coefficients on a sample of only Caucasian individuals. Standard errors are clustered at the county level and confidence intervals are drawn at 99 percent. For the Hispanic sample,  $N = 52,222$  whereas for the Caucasian sample  $N = 133,907$ .

### 3.8 Conclusion

Undocumented migration in the United States has become a deeply polarized issue. In this chapter, we set out to investigate the distributional consequences of giving undocumented migrants legal status through a nation-wide amnesty program. Our contention has been that state governors allocate more resources to those counties where the newly documented migrants reside in an effort to win over their future political support. We substantiated this hypothesis in parts.

First, we found that documenting migrants does indeed have a significant dis-



tributional component. Across a number of specifications, our results consistently demonstrate that as the share of documented migrants in a county increases, so too does the amount of per capita aid received by that county from its respective state government.

Second, in trying to understand why legal status affects the distribution of public finances, we uncovered political economy forces at work. We presented evidence that the allocation of state aid that arises out of the IRCA varies significantly according to the political context in which an incumbent governor finds him or herself. Governors transfer more resources to IRCA-affected counties when the governor is eligible for re-election, when the county is more politically contested, when the governor enjoys line-item veto power over the state budget and when the legislative and executive branches of state government are politically aligned. These results are especially noteworthy as it suggests that the relationship between legal status and the distribution of public resources is one of discretionary political choice rather than one of economic necessity or mechanical welfare increases.

In the final part of our analysis, we addressed the question of whether—and to what extent—state governors actually targeted resources to capture the political support of the newly documented migrants rather than that of other, perhaps competing, voting groups. In this respect, we exploited data from a key anti-migrant ballot measure as well as from survey data on attitudes and found little evidence of anti-migrant sentiment confounding our results. Lastly, we found that county expenditure in education increases significantly in IRCA-affected counties and that, consequently, Hispanic individuals, as compared to Caucasian ones, residing in those same counties experience significant improvements in educational outcomes, further suggesting that the resource allocation arising out of the IRCA is intended to service the needs, and win the political support, of the newly documented migrants.

On the whole, then, our findings point to a significant political economy dimension to immigrant legalization. Offering legal status not only leads to various social and economic improvements at the local level but also provides politicians with strong electoral incentives to see that it does so.

## Appendix C

### C.1 Analysis of the Model

Our analysis begins by taking first order conditions of Equation 3.1 with respect to  $g$ :

$$\left. \frac{\partial \Pi}{\partial g} \right|_{g=g^*} = (1 - \alpha)U'_C(g^*) + \alpha U'_L(g^*) + E \cdot \frac{\partial \Omega}{\partial \phi} \cdot \frac{\partial \phi}{\partial g^*} - C'(g^*) \stackrel{!}{=} 0 \quad (3.8)$$

To understand how  $g^*$  responds to a sudden shock in legal status,  $\alpha$ , we maximize 3.8 and this is implicitly given by the following:

$$\frac{\partial g^*}{\partial \alpha} = - \frac{U'_L(g^*) - U'_C(g^*) + E \cdot \frac{\partial \frac{\partial \Omega}{\partial \phi} \frac{\partial \phi}{\partial g}}{\partial \alpha}}{\left. \frac{\partial^2 \Pi}{\partial g^2} \right|_{g=g^*}} \quad (3.9)$$

Because  $\frac{\partial^2 \Pi}{\partial g^2} < 0$  the sign in front of Equation 3.9 becomes positive. Moreover, we have assumed that  $U'_L(g) > U'_C(g) \forall g$ ; accordingly, the first term in the numerator,  $U'_L(g^*) - U'_C(g^*) > 0$ . The overall sign of Equation 3.9 thus hinges on the sign of the second term in the numerator which can be expressed as follows:

$$\frac{\partial \frac{\partial \Omega}{\partial \phi} \frac{\partial \phi}{\partial g}}{\partial \alpha} = \frac{\partial \frac{\partial \Omega}{\partial \phi}}{\partial \alpha} \cdot \frac{\partial \phi}{\partial g} + \frac{\partial \Omega}{\partial \phi} \cdot \frac{\partial \frac{\partial \phi}{\partial g}}{\partial \alpha} \quad (3.10)$$

Rewriting  $\frac{\partial \frac{\partial \Omega}{\partial \phi}}{\partial \alpha} = \frac{\partial \frac{\partial \Omega}{\partial \phi}}{\partial \alpha} \cdot \frac{\partial \phi}{\partial \phi} = \frac{\partial^2 \Omega}{\partial \phi^2} \cdot \frac{\partial \phi}{\partial \alpha}$ , and  $\frac{\partial \frac{\partial \phi}{\partial g}}{\partial \alpha} = \frac{\partial \frac{\partial \phi}{\partial g}}{\partial \alpha} \cdot \frac{\partial g}{\partial g} = \frac{\partial^2 \phi}{\partial g^2} \cdot \frac{\partial g}{\partial \alpha}$  we can substitute these back into Equation 3.10 to obtain:

$$\begin{aligned} &= \frac{\partial^2 \Omega}{\partial \phi^2} \cdot \frac{\partial \phi}{\partial \alpha} \cdot \frac{\partial \phi}{\partial g} + \frac{\partial \Omega}{\partial \phi} \cdot \frac{\partial^2 \phi}{\partial g^2} \cdot \frac{\partial g}{\partial \alpha} \\ &= \frac{\partial^2 \Omega}{\partial \phi^2} \cdot \frac{\partial \phi}{\partial \alpha} \cdot \frac{\partial \phi}{\partial g} \cdot \frac{\partial g}{\partial g} + \frac{\partial \Omega}{\partial \phi} \cdot \frac{\partial^2 \phi}{\partial g^2} \cdot \frac{\partial g}{\partial \alpha} \\ &= \frac{\partial^2 \phi}{\partial g^2} \cdot \frac{\partial g}{\partial \alpha} \cdot \left( \frac{\partial^2 \Omega}{\partial \phi^2} + \frac{\partial \Omega}{\partial \phi} \right) \end{aligned}$$

Under the assumption that  $\frac{\partial \Omega}{\partial \phi} > |\frac{\partial^2 \Omega}{\partial \phi^2}|$ , the overall sign of Equation 3.10 is thus positive. This in turn allows us to state that  $\frac{\partial g^*}{\partial \alpha} > 0$ .<sup>38</sup>

<sup>38</sup> Although we have used a general functional form for  $\Omega$ , for illustrative purposes, we set  $\Omega(\phi) = \frac{1}{1+e^{-\phi}}$  and plot the various derivatives of  $\Omega(\phi)$ , shown in Figure C.1, to provide some intuition behind this assumption.

**Prediction 1:** The optimal allocation of state aid increases in the share of newly documented migrants in a county.

Given that the second term in the numerator in Equation 3.9 is positive, we can state that  $\left. \frac{\partial g^*}{\partial \alpha} \right|_{E=1} > \left. \frac{\partial g^*}{\partial \alpha} \right|_{E=0}$ .

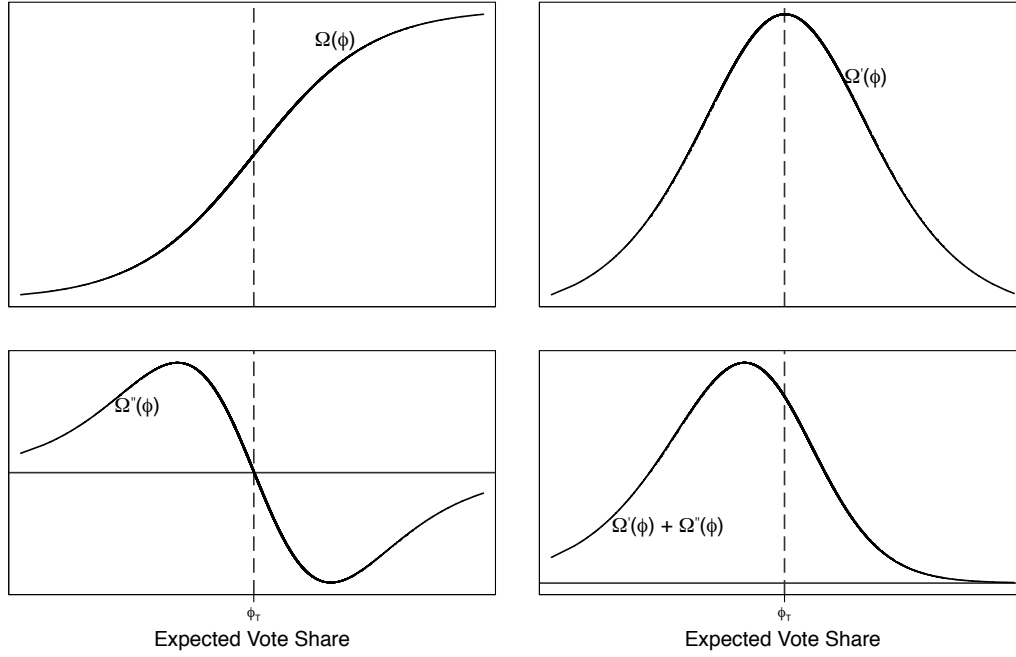
**Prediction 2:** The optimal allocation of state aid is larger when  $P$  is eligible for re-election and less when (s)he is a lame duck.

Finally, the functional form of  $\Omega(\phi)$  leads us to a final testable prediction. Because  $\phi = \phi_T$  represents an inflection point (where  $\phi_T$  represents the winning threshold of an election), it follows that  $\frac{\partial \Omega}{\partial \phi}$  is maximized as  $\phi \rightarrow \phi_T$ .

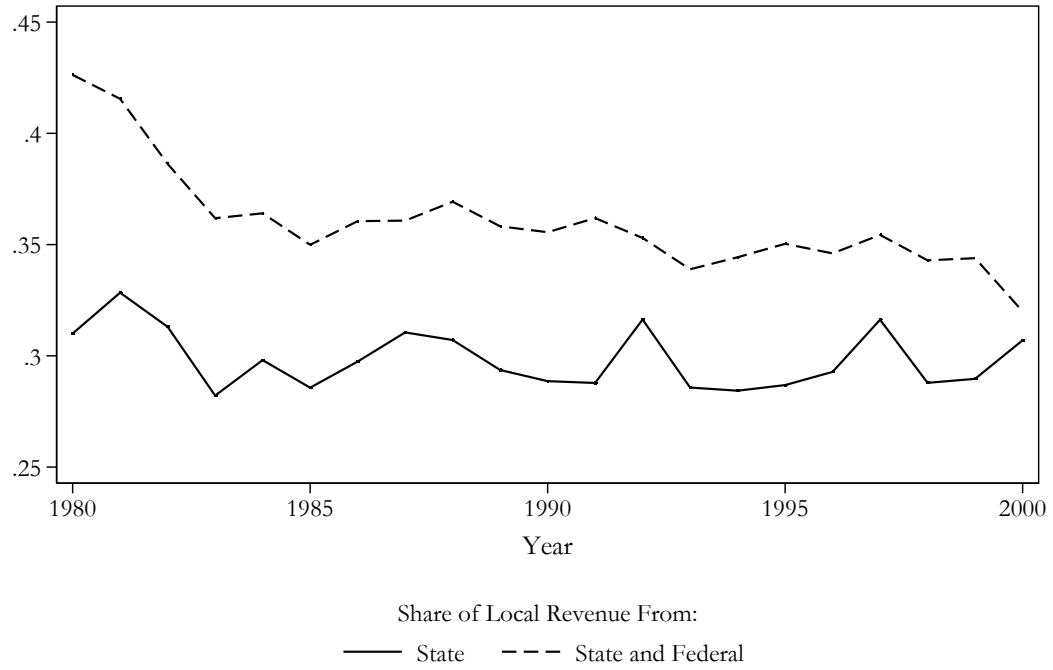
**Prediction 3:** The optimal allocation of state aid becomes larger the closer the  $P$ 's expected vote share is to the winning threshold.

## C.2 Additional Figures

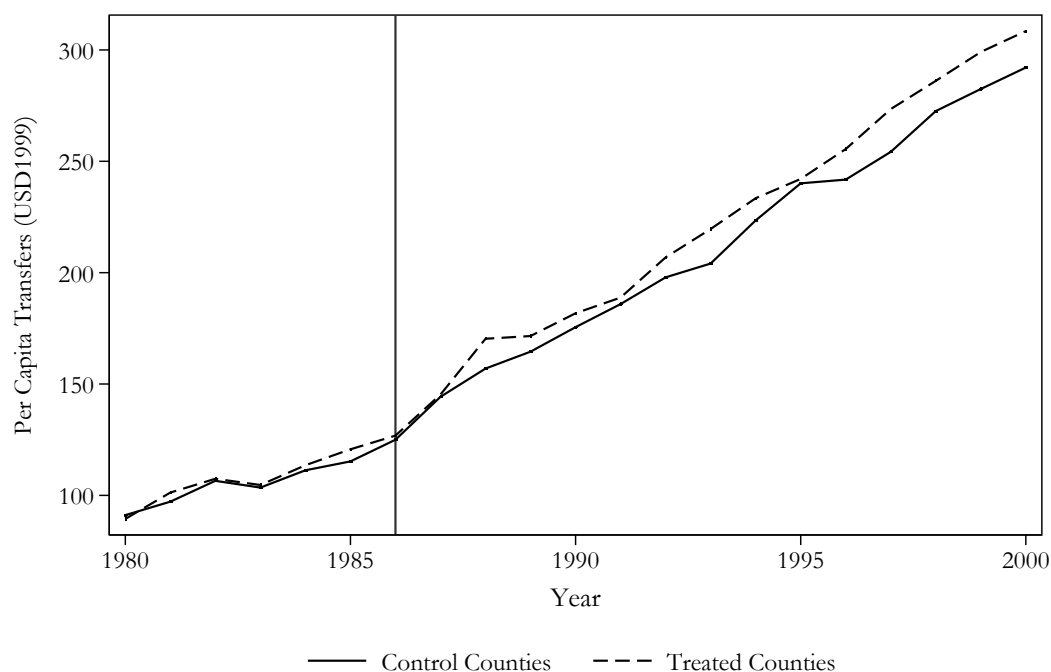
**Figure C.1:** Functional form of  $\Omega(\phi)$ , its first and second derivative and their sum



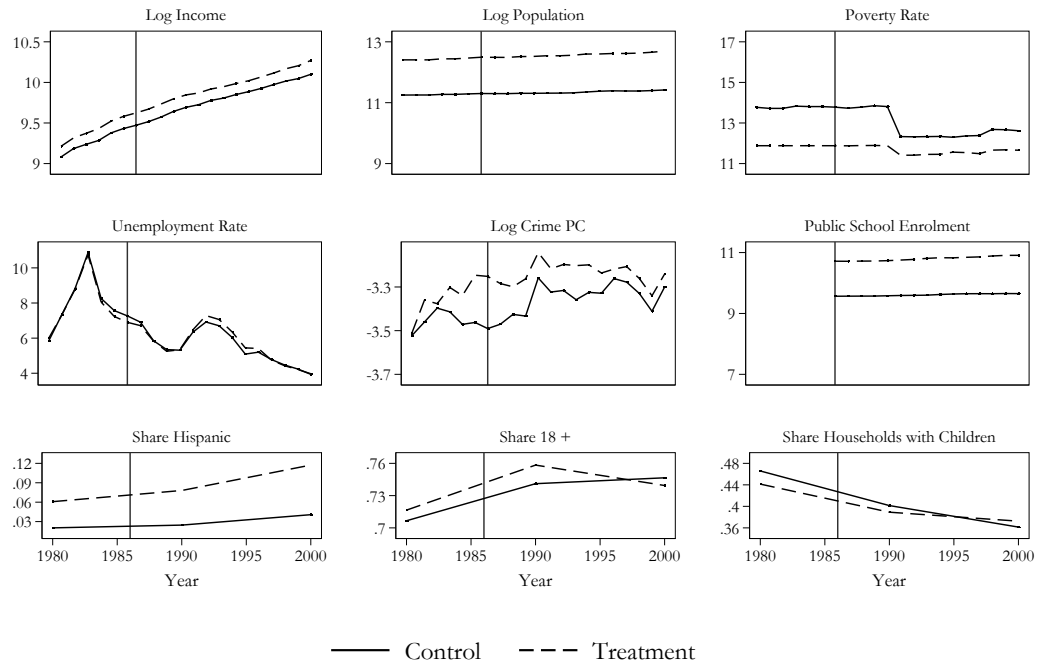
Notes: This graph plots, clockwise from top-left:  $\Omega(\phi)$ ;  $\Omega'(\phi)$ ;  $\Omega''(\phi)$ ; and  $\Omega'(\phi) + \Omega''(\phi)$

**Figure C.2:** Sources of local government revenue

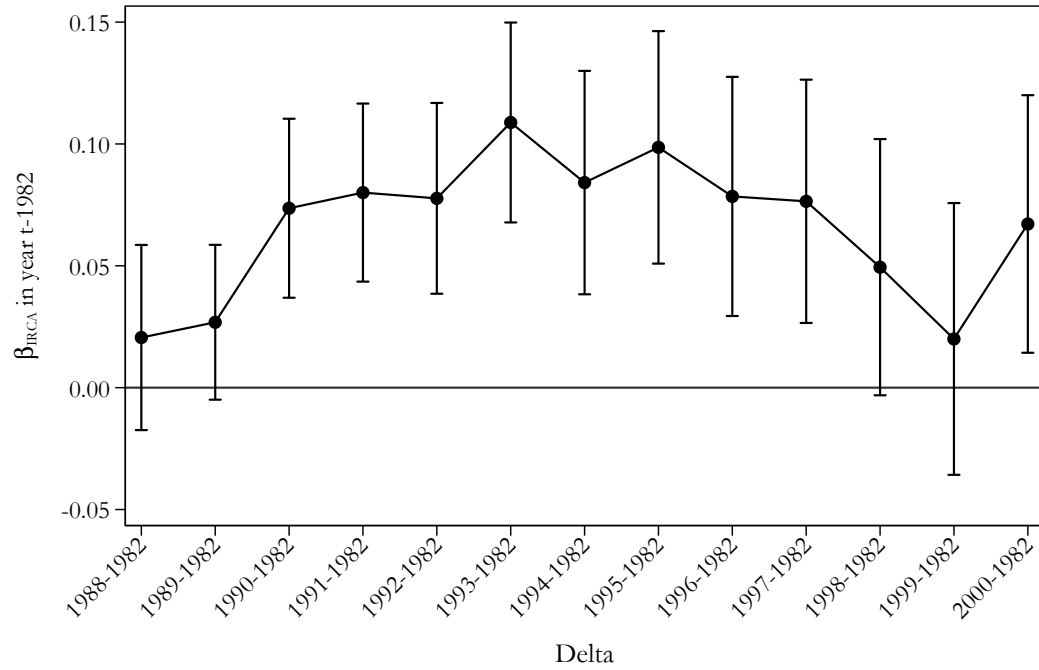
Notes: This graph plots the share of local government revenue (cities, municipalities and counties aggregated to the county) coming from state transfers and state and federal transfers.

**Figure C.3:** Evolution of inter-governmental revenues in matched sample

Notes: This graph compares per capita inter-governmental revenues (in USD1999) in those counties that never received applications for legal status (control) with those counties that did receive applications for legal status (treated) through the IRCA in a sample of treated and control counties matched on the basis of propensity scores using the nearest neighbour. The county characteristics on which we base the propensity score matching are county income, population, crime, tax revenue, poverty rate and unemployment in 1980.

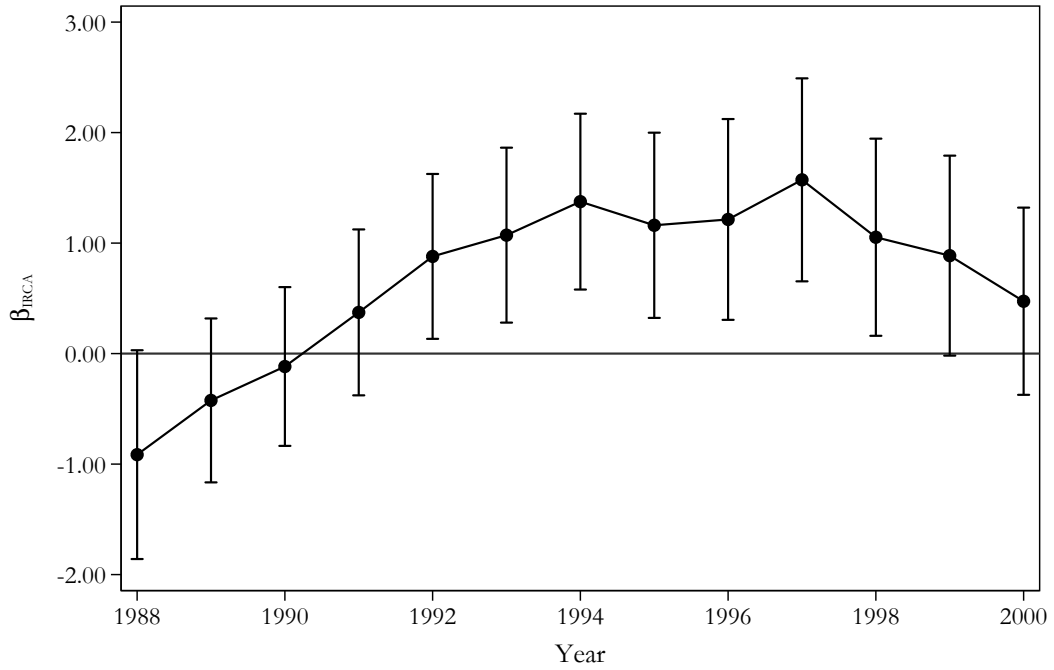
**Figure C.4:** Trends in county socio-economic characteristics

Notes: This graph compares the evolution of various county characteristics in those counties that never received applications for legal status (control) with those counties that did receive applications for legal status (treated) through the IRCA.

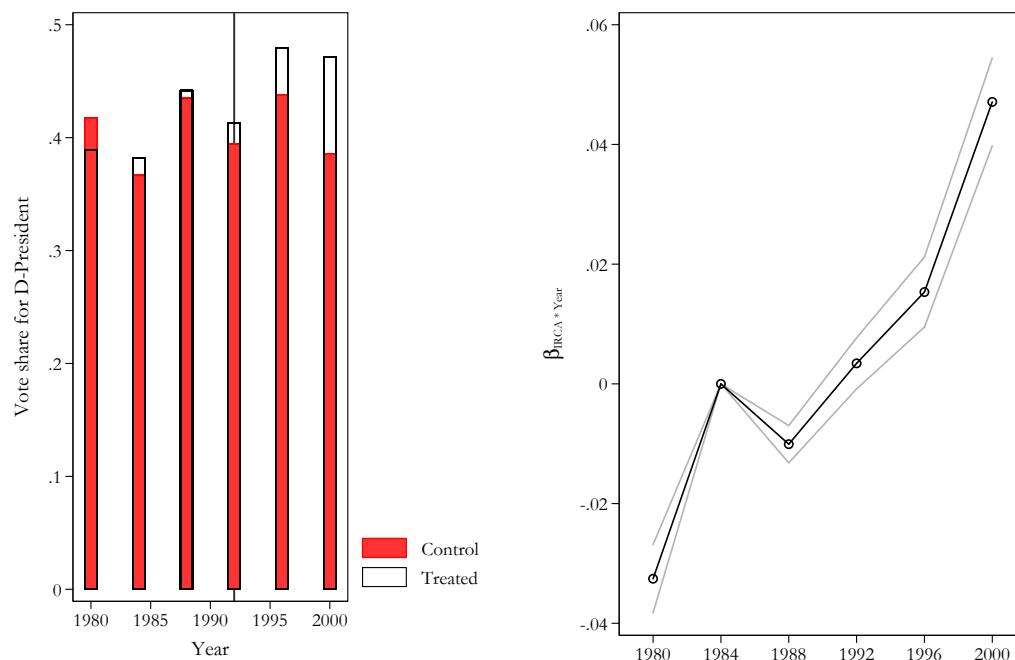
**Figure C.5:** First-difference coefficient estimates

Notes: This graph plots the coefficients from various first-difference regressions from 1988 to 2000 using 1982 as the base year. The dependent variable is the log of per capita transfers from state to local governments (in 1999 USD) and  $\beta$  is the coefficient on the natural log of the cumulative number of IRCA applicants per 1000 county inhabitants (plus one). Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. County fixed effects and state-year fixed effects are also included in the estimations. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent.

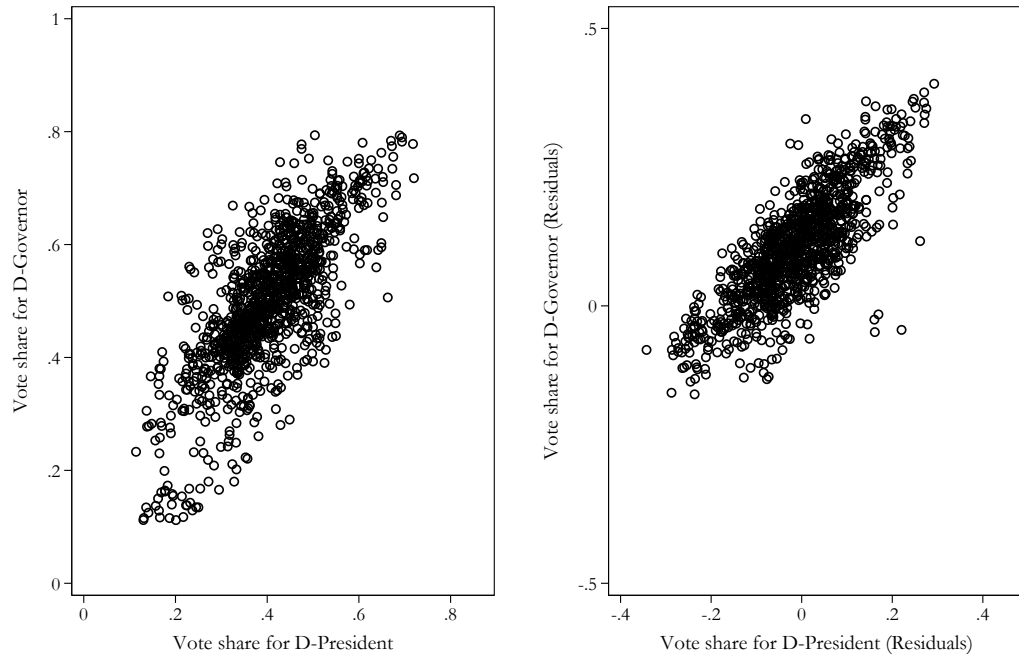


**Figure C.6:** Instrumental variables regression coefficients

Notes: This graph plots  $\beta$  from 13 cross section regressions as specified in Equation 3.5, one regression each for the years between 1988 and 2000. For each year, the value of the covariates is differenced from their 1982 value. The dependent variable is the log of per capita transfers from state to county governments (in 1999 USD) and  $\beta$  is the coefficient on the natural log of the cumulative number of IRCA applicants per 1000 county inhabitants (plus one) when it is instrumented by the 1960 share of a county that is foreign-born. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent.

**Figure C.7:** The IRCA and the Democratic vote share

Notes: The panel on left plots the Democratic vote share at the county level in Presidential elections in counties affected by the IRCA against those not affected by the IRCA. The panel on the right shows coefficients from a regression where Democratic vote share (in Presidential elections) is regressed on an interaction between a treatment indicator and year. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level as well as county fixed effects and state-by-year fixed effects. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent.  $N = 12,754$

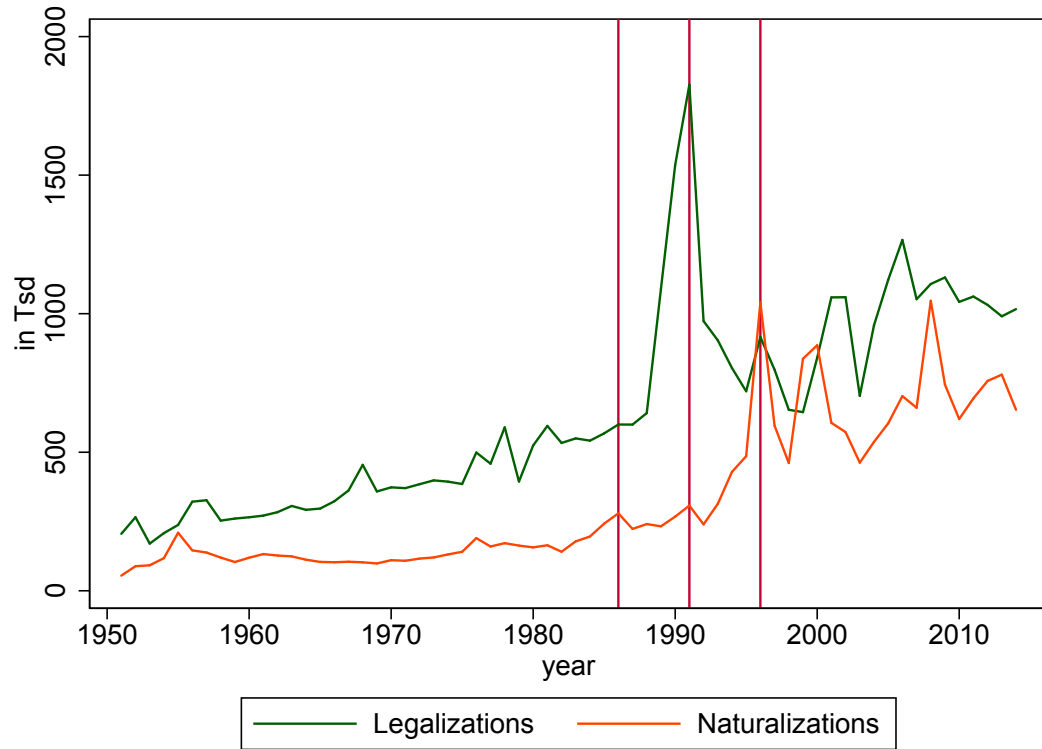
**Figure C.8:** Presidential and gubernatorial election results

Notes: These figures plot the Democratic vote share at the county level in Presidential and Gubernatorial elections beginning in 1992. The scatter on the left plots the raw data while the scatter on the right plots the variables once state-year fixed effects and county fixed effects have been accounted for.

**Figure C.9:** Governor veto power index over time

Notes: This graph plots an index of veto power enjoyed by state governors over time. The index is interpreted as follows: 5 Governor has item veto and a special majority vote of the legislature is needed to override a veto [three-fifths of the legislators elected or two-thirds of the legislators present]; 4.5 Governor has item veto, with a majority of legislators elected needed to override, except for appropriations bills when votes of two-thirds of those elected are needed to override; 4 Governor has item veto with a majority of legislators elected needed to override; 3 Governor has item veto with only a majority of legislators present needed to override; 2 Governor has no item veto, with a special legislative majority needed to override; 1 Governor has no item veto, with only a simple majority needed to override; 0 Governor has no veto of any kind.

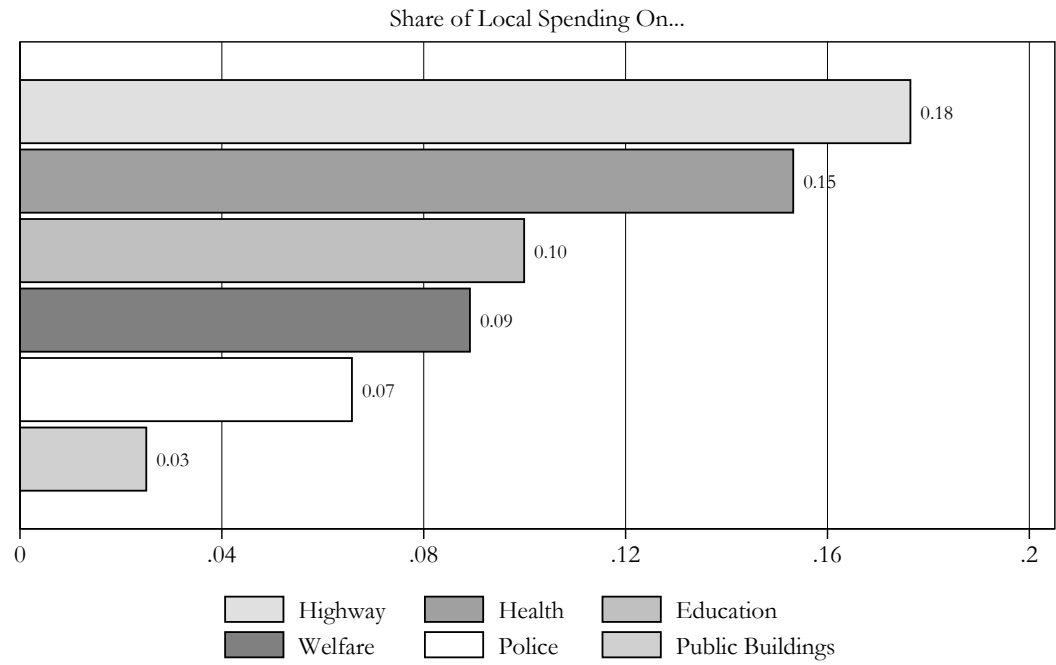
Source: Data on the institutional power ratings for the governorships in the U.S are provided by Thad L. Beyle: [https://www.unc.edu/polisci\\_old/beyle/gubnewpwr.html](https://www.unc.edu/polisci_old/beyle/gubnewpwr.html). Accessed 16 April 2018.

**Figure C.10:** Naturalization and legalization at the state level

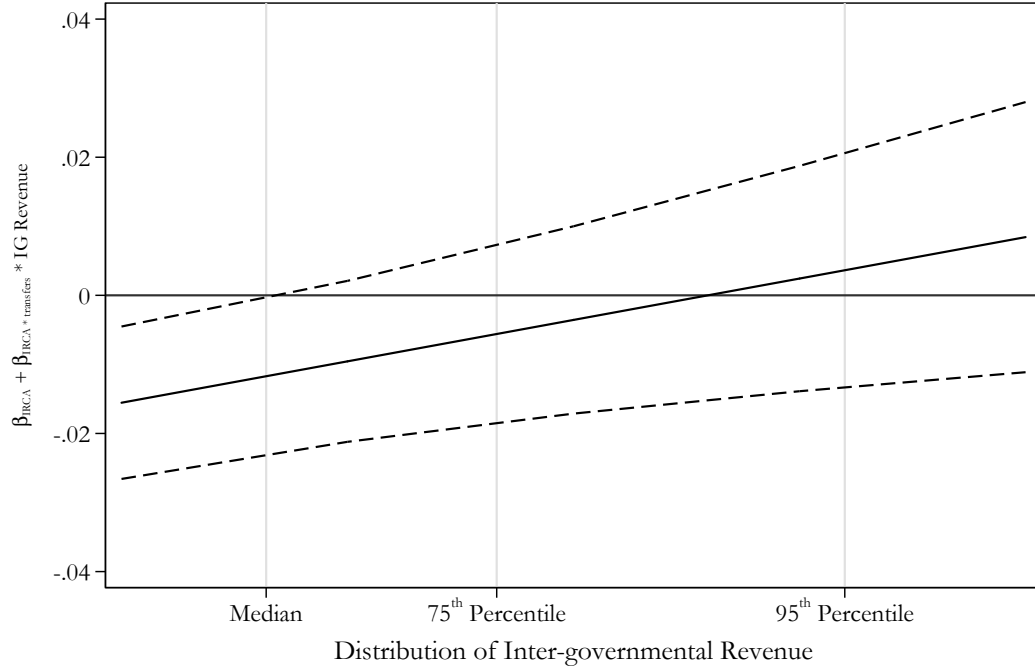
Notes: This graph plots trends in naturalizations and legalizations at the state level. Lines drawn at 1986 (the passage of the IRCA), 1992 (first cohort of naturalizations arising from the IRCA) and 1996.

Source: Immigration and Naturalization Services, own data.

**Figure C.11:** Share of local expenditure on...



Notes: This graph plots various categories of local government expenditure as a share of total local expenditure.

**Figure C.12:** Marginal effect of the IRCA on the propensity to vote

Notes: This graph plots the marginal effect of immigrant legalization on the propensity to vote when immigrant legalization is interacted with inter-governmental revenue from the state. In other words, it plots  $\frac{\partial \text{vote}}{\partial \text{IRCA}} = \beta_{\text{IRCA}} + \beta_{\text{IRCA} \times \text{transfers}} * \text{transfers}$  along the distribution of inter-governmental revenue.  $\beta_{\text{IRCA}}$  is the coefficient on the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). The regression draws on individual data from the 1996, 1998 and 2000 November Voter Supplement of the CPS. The outcome variable is an indicator that is one if an individual voted in that year's November election and zero otherwise. Control variables include individual race, sex, family income, marital status, education and age as well as year dummies and county population. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent.  $N = 41,968$ .

### C.3 Additional Tables

**Table C.1:** Congressional vote record on the IRCA bill

	House	Senate
Yes	274 (204-D; 70-R)	63 (34-D; 29-R)
No	132 (33-D; 99-R)	24 (5-D; 8-R)
Abstain	26	13

Notes: This table shows how the 99<sup>th</sup> Congress voted for the IRCA Bill on 17 October 1986.

Source: Congressional Votes Database accessed at [govtrack.us](http://govtrack.us). Accessed 20 October 2018.



**Table C.2:** Inter-governmental revenue from state to local governments: Categories of revenue

	Education	Health and Hospitals	Highways	Public Welfare
Includes	State aid for support of local schools; redistribution of federal aid for education; handicapped, special, and vocational education and rehabilitation; student transportation; equalization aid; school health; local community colleges; adult education; school buildings; and property tax relief related strictly to school funding.	State aid for local health programs; maternal and child health; alcohol, drug abuse, and mental health; environmental health; nursing aid; hospital financing (including construction); and hospitalization of patients in local government hospitals.	State aid for construction, improvement, or maintenance of streets, highways, bridges, tunnels, etc.; distribution of state fuel taxes; and aid for debt service on local highway debt.	State aid for public welfare purposes; medical care and related administration under public assistance programs (including Medicaid) even if received by a public hospital; care in nursing homes not associated with hospitals; federal categorical assistance (e.g., pass through of Aid to Families with Dependent Children, or AFDC); and administration of local welfare programs.
Excludes	State grants for libraries; state expenditures on behalf of local schools for textbooks, buses, school buildings, etc.; and value of donated food commodities (non-revenue).	State aid for medical care under public assistance programs such as Medicaid.	State grants for urban mass transit	

Notes: This table explains for what purposes inter-governmental revenue from state to local governments (counties, cities, municipalities aggregated to the county) is used for. We only observe these revenues in aggregate at the county level and do not observe the categories. This information is simply informative to give the reader an idea of the sorts of things a state governor can and cannot support with state-to-county transfers.

Source: Information taken from The Census Government Finance and Employment Classification Manual which can be accessed at: <https://www.census.gov/govs/www/classrevdef.html>

**Table C.3:** Baseline results with alternative clustering and inference

	(1) Treatment $\times$ Post	(2) Legalization Intensity
$\hat{\beta}$	0.0709	0.0610
<i>p</i> -values:		
A. Analytical values (clustered at the state level)	0.0325	0.0149
B. Wild Bootstrap values (clustered at the state level)	0.0521	0.0390
Observations	46,820	46,820
Number of States	46	46

Notes: This table presents the baseline estimates (column (1) of Table 3.2) clustering the standard errors at the state level. *p*-values are derived both analytically, using Stata's conventional `vce(cluster state)` command as well as through Wild cluster bootstrapping generated using Roodman et al. (2018) `boottest` command.

**Table C.4:** Baseline results using linear legalizations

	Log of Inter-governmental Revenue (per capita)				
	(1)	(2)	(3)	(4)	(5)
	Baseline	Drop Top 5	Pop < 409,490	Matching	Linear Trends
Legalizations per 1,000 capita	0.00551*** (0.00149)	0.0129*** (0.00332)	0.00370*** (0.00140)	0.00557*** (0.00164)	0.00412*** (0.00158)
Control Variables	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	No	Yes
Linear Year Trends	No	No	No	No	Yes
Observations	46,820	43,952	45,132	12,042	46,820
Number of Counties	2,686	2,526	2,612	604	2,686

Notes: This table replicates the baseline estimation reported in Panel B of Table 3.2 but using the cumulative number of IRCA applications from a given county in a given year per 1000 county inhabitants as the key independent variable without a log transformation. The dependent variable is the log of per capita transfers from state to local governments aggregated to the county in 1999 USD. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table C.5:** Democratic vote share on IRCA legalizations

	Democratic Vote Share		
	(1) All States	(2) D State	(3) R State
Log legalizations <sub><i>t</i>-5</sub>	0.0204*** (0.00175)	0.0223*** (0.00333)	0.0188*** (0.00206)
Control Variables	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes
Observations	7789	4203	3586
Number of Counties	2141	1127	1014

Notes: The dependent variable is the Democratic vote share, at the county level, in Presidential elections. **Log legalizations<sub>*t*-5</sub>** is a five year lag of the log of the cumulative number of IRCA applications from a given county in a given year per 1000 county inhabitants (plus one). In columns (2) and (3) we split the sample and look at the relationship only in Democratic (column (2)) or Republican (column (3)) states defined by the party of the Governor in the year 1990. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table C.6:** Legalization and dynamics of the 99<sup>th</sup> congress

	Log of Inter-governmental Revenue (per capita)	
	(1)	(2)
Log legalizations	0.0442** (0.0221)	0.0444* (0.0248)
Log legalizations $\times$ Majority D-Senators in State	0.0201 (0.0275)	
Log legalizations $\times$ Majority D-Members in State		0.0189 (0.0293)
Control Variables	Yes	Yes
County Fixed Effects	Yes	Yes
State-Year Fixed Effects	Yes	Yes
Observations	46,820	46,820
Number of Counties	2,686	2,686

Notes: The dependent variable is the log of per capita transfers from state to county governments in 1999 USD. **Log legalizations** is the log of the cumulative number of IRCA applications from a given county in a given year per 1000 county inhabitants (plus one). **Majority D-Senators in State** is an indicator that is 1 if both senators of a given state are Democrats and 0 otherwise. **Majority D-Members in State** is defined similarly: it is 1 if the majority of congress members from a given state were Democrats and 0 otherwise. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table C.7:** Legalization and term limits

	Log of IGR (per capita)
	Incentive
Log legalizations	0.254 (0.197)
Log legalizations $\times$ Electoral Incentive	0.0923*** (0.0241)
Control Variables	Yes
County Fixed Effects	Yes
State-Year Fixed Effects	Yes
Observations	12,134
Number of Counties	2,384

Notes: The dependent variable is the log of per capita transfers from state to county governments in 1999 USD. **Log legalizations** is the log of the cumulative number of IRCA applications from a given county in a given year per 1000 county inhabitants (plus one). **Electoral Incentive** is an indicator that is 1 if a governor is not a lame duck in the period between 1989 and 1994 and zero otherwise. The baseline effect of **Electoral Incentive** is captured by state-year fixed effects and is thus unable to be estimated. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table C.8:** Local spending and IRCA legalizations

	Log of Per Capita Local Expenditure				
	(1) Total	(2) Health	(3) Education	(4) Welfare	(5) Highway
Log of Transfers	0.254*** (0.0184)	0.285*** (0.0228)	0.0435*** (0.0113)	0.205*** (0.0199)	0.245*** (0.0193)
Control Variables	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	36,869	36,869	36,869	36,869	36,869
Number of Counties	2,638	2,638	2,638	2,638	2,638

Notes: This table presents regression results using various categories of per capita local government expenditure as the outcome variable. **Log of Transfers** is the per capita inter-governmental revenue from the state to local governments aggregated to the county in 1999 USD. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table C.9:** IRCA, citizenship and voter turnout using CPS

	(1) Naturalized	(2) Voted in Nov	(3) Voted in Nov
Log legalizations	0.0221*** (0.0046)	-0.0060 (0.0051)	-0.0770*** (0.0207)
Log of Transfers, pc 1999			-0.0086 (0.0085)
Log Legalizations $\times$ Log Transfers			0.0126*** (0.0036)
Individual Controls	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	50206	46210	41968

Notes: This table presents regression results using individual data from the 1996, 1998 and 2000 November Voter Supplement of the CPS. **Log legalizations** is the log of the cumulative number of IRCA applications from a given county in a given year per 1000 county inhabitants (plus one). **Log Transfers** is the per capita inter-governmental revenue from the state government to the county in 1999 USD. Column (1) has an outcome variable that is 1 if an individual is a naturalized citizen and 0 if (s)he is a native citizen. Columns (2) and (3) have as outcome variables indicators that are 1 if an individual voted in that year's November election and 0 if (s)he did not. The years for which we have data include three major gubernatorial election cycles as well as two Presidential elections but we cannot distinguish which election an individual voted for. Control variables include individual race, sex, family income, marital status, education and age. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

### State Legalization Impact Assistance Grants (SLIAG)

Section 204 of the IRCA outlines the details associated with the State Legalization Impact Assistance Grants (SLIAG)—a \$1B per year federal funding program for four years which could be spent over seven years until 1994. SLIAG was designed to compensate states for the extra costs they would incur as a result of the legalization program of the IRCA. Specifically, SLIAG funds were intended to assist states to defray expenses in the areas of public health, public assistance and education (Liu, 1991; DHHS, 1991). It is unlikely that the SLIAG funds are confounding our results for the simple reason that SLIAG was administered through an entirely separate institutional set-up and is not part of the inter-governmental revenue outcome variable that we exploit.<sup>39</sup> Nevertheless, we obtain the amount of funding states received from SLIAG and deduct it from our main outcome variable to create an ‘inter-governmental revenue net of SLIAG’ variable. Results are shown in Table C.10 and confirm that the resource allocation to affected counties is not being confounded by SLIAG funds.

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<sup>39</sup> As part of the IRCA, The federal government instituted the Single Point of Contact (SPOC) system, whereby every state designated its own SPOC so as to create “state-level lead implementation agencies to manage the [SLIAG] program according to the unique needs and arrangements of the individuals states” Liu (1991). At the federal level, it was the Department of Health and Human Services that received applications for and disbursed the SLIAG funds but SLIAG required that SPOCs coordinate directly with state and local public health, public assistance and education organizations to receive the funds Liu (1991).



**Table C.10:** Transfers on IRCA legalizations net of SLIAG funds

	(1) Net Transfers PC
Log legalizations	0.398*** (0.0807)
Control Variables	Yes
Year Fixed Effects	Yes
County Fixed Effects	Yes
State-Year Fixed Effects	Yes
Observations	6,490
Number of Counties	2238

Notes: The dependent variable is the log of per capita transfers from state to county governments in 1999 USD net of SLIAG funds received from the federal government. **Log legalizations** is the log of the cumulative number of IRCA legalized migrants in a given county in a given year per 1000 county inhabitants (plus one). SLIAG was made available until 1991 and so our net transfers variable is defined only until that period which explains the smaller number of county-year observations. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$



## Chapter 4

# Price Transparency Against Market Power<sup>\*</sup>

### Abstract

We study under what conditions mandatory price disclosure is pro- or anti-competitive. Using a theoretical search model with collusion and imperfectly informed consumers and producers, we characterize the circumstances under which increasing price transparency benefits consumers. We show that the level of ex ante consumer and producer transparency, as well as the number of consumers adopting the mandatory price information, determine whether the policy is pro- or anti-competitive. To examine the theoretical predictions, we construct a unique data set to study the effect of mandatory price disclosure in the German petrol market. We find that the policy led to a decrease in retail margins by 13 percent. We also find that the theoretical model is better at explaining the relationship between the treatment effect and ex ante consumer transparency than other models in the literature. Our results inform policymakers by highlighting how the effect of mandatory price disclosure depends on market characteristics.

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<sup>\*</sup> This chapter is based on joint work with Felix Montag.

## 4.1 Introduction

Many firms derive their market power, and thus their ability to price above the perfectly competitive level, from informational frictions. At the same time, modern information technologies allow the collection and diffusion of vast amounts of information at low costs. In some markets, price comparison platforms have formed, while in others they have not. One obstacle for the creation of price comparison platforms is the cost of collecting prices. If policymakers want to foster the creation of such platforms, they can help overcome this issue by mandating firms to disclose prices. However, the existing theoretical literature shows that in markets prone to collusion increasing price transparency among consumers and producers can harm consumers.<sup>1</sup>

In this paper, we ask under what conditions mandatory price disclosure is likely to be pro- or anti-competitive. The theoretical literature shows that small changes to the underlying assumptions often lead to very different relationships between price transparency and the stability of collusive behavior. The empirical literature on mandatory price disclosure also found different results in different contexts.<sup>2</sup> Understanding how different market conditions affect the effect of mandatory price disclosure is thus crucial for the successful design of future policies.

To answer our research question, we begin by setting up a theoretical model of collusion, which builds on the homogeneous goods models by Petrikaite (2016) and Schultz (2017).<sup>3</sup> Using this model, we derive predictions on how increasing price transparency on both sides of the market affects the stability of collusive behavior and contrast these with the predictions of other candidate models.<sup>4</sup> Next, we study the introduction of mandatory price disclosure in the German retail petrol market empirically and assess the suitability of the theoretical model. We begin by constructing a novel data set of station-level retail margins in Germany and France before and after the introduction of mandatory price disclosure. We also use administrative data on the universe of commuters in Germany to construct a proxy for the ex ante level of consumer transparency. Then, we estimate the average effect of mandatory price disclosure in Germany. Thereafter, we study how an increase in the intensity of the treatment for consumers due to follow-on local media reports affects retail margins. Finally, we analyze heterogeneities in the treatment effect according to the ex ante

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<sup>1</sup> See, for example, Kühn and Vives (1995), Schultz (2017) or Luco (2019).

<sup>2</sup> See, for example, Luco (2019), who finds that mandatory price disclosure increased retail margins in the Chilean petrol market and Ater and Rigbi (2018), who find that mandatory price disclosure decreased prices in the Israeli supermarket industry.

<sup>3</sup> Other theoretical models, such as by Luco (2019), rely on modeling goods as heterogeneous.

<sup>4</sup> We focus on other theoretical models of imperfectly informed producers and consumers that allow for collusionary behavior, namely the models by Schultz (2017) and Luco (2019).

level of consumer transparency to assess the suitability of various models discussed in the literature.

In the theoretical model, we assume that producers and consumers are imperfectly informed and producers may engage in collusive behavior. In contrast to Petrikaite (2016), we allow producers to be imperfectly informed. In contrast to Schultz (2017), we allow uninformed consumers to search sequentially. These assumptions tailor the model to the dynamics of the petrol market, whilst making it more general, since both models are nested within the model in this paper. In line with the literature, we find that the effect of mandatory price disclosure depends on how much this changes the levels of producer and consumer transparency. In contrast to existing models with imperfect information on both sides of the market but without sequential search, we show that whether a marginal increase in transparency is pro- or anti-competitive is determined by the ex ante levels of transparency. Our framework allows us to analyze how increasing a common factor of transparency, affecting consumer and producer transparency, affects the stability of collusionary behavior.

Since the assumptions of the different models affect the mechanisms and policy recommendations, it is important to assess which model best fits a particular market. We therefore derive theoretical predictions of the model that we can verify empirically and contrast these with the predictions of other candidate models.<sup>5</sup> In particular, we highlight how the different models have different predictions about the marginal treatment effect, which depend on the level of ex ante consumer and producer transparency.

To validate the predictions of the model, we study the introduction of the Market Transparency Unit for Petrol (MTU) in Germany. Since September 2013, a law forces petrol stations in Germany to disclose all price changes in real-time to a central database that allows information service providers to diffuse this information to consumers. Using a difference-in-differences approach, we causally estimate the effect of mandatory price disclosure on retail margins. We do this by comparing retail margins for the universe of petrol stations in France and Germany for the years 2013 and 2014.

This setting is particularly suitable because it allows us to overcome two important empirical challenges: Firstly, using 11 million price notifications to a smartphone application collecting consumer-reported petrol prices allows us to observe prices before the introduction of mandatory price disclosure.<sup>6</sup> We combine this with price data

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<sup>5</sup> We focus on other theoretical models of imperfectly informed producers and consumers that allow for collusionary behavior, namely the models by Schultz (2017) and Luco (2019).

<sup>6</sup> There already existed some apps that allowed users to self-report petrol prices, which were then collected and diffused to users in a similar fashion to the price information from the MTU. However, the usage of these apps before the MTU was considerably lower than after its introduction. This is why the introduction of the MTU led to an important change in the the information set of market participants.

from the MTU after its introduction. We complement the data with station-level retail margins for France over the full observation period. Secondly, we can provide empirical evidence on the predictions of the theoretical model and assess which of the candidate models best explains the empirical evidence. To do so, we construct a proxy of the ex ante level of consumer transparency using administrative data on the universe of commuters, following the approach by Pennerstorfer et al. (2019). This allows us to study the treatment effect for different levels of ex ante consumer transparency. Additionally, we construct a dataset on radio reports by local radio stations about petrol prices. Using geocoded reception areas, we can identify whether a petrol station lies within the reception area of a radio station. This allows us to study whether follow-on information campaigns led to a further decrease in retail margins.

Overall, we find that the introduction of mandatory price disclosure in Germany led to a decrease in retail margins by approximately 1 Eurocent, or 13 percent of retail margins.<sup>7</sup> We also find that average retail margins decrease by a further 0.6 Eurocent if a complementary information campaign, proxied by local radio reports about petrol prices, accompanies mandatory price disclosure. As predicted by the theoretical model, we find that the level of ex ante consumer transparency leads to significant heterogeneities in the marginal treatment effect. The empirical evidence suggests a downward-sloping relationship between the level of ex ante consumer transparency and the treatment effect, which means that the higher the level of ex ante consumer transparency, the larger the magnitude of the treatment effect. This result is consistent with predictions of the theoretical model and at odds with predictions of other candidate models.

The paper makes three main contributions. Firstly, we extend the theoretical literature on price transparency when there is imperfect information on both sides of the market and firms may engage in collusive behavior.<sup>8</sup> There is a rich theoretical literature on collusion under imperfect information on the side of producers (e.g. Green and Porter, 1984 or Kühn and Vives, 1995) and consumers (e.g. Schultz, 2005, Rasch

<sup>7</sup> According to German Statistical Office, private households consumed 46 billion liters of fuel in Germany in 2016. A crude back-of-the-envelope calculation shows that the decrease in margins caused by mandatory price disclosure in the German petrol market could thus lead to savings of 460 million Euro to private households every year. Data available at <https://www.destatis.de/DE/Themen/Gesellschaft-Umwelt/Umwelt/Materialfluesse-Energiefluesse/Tabellen/fahrleistungen-haushalte.html>. Accessed 26 October 2018.

<sup>8</sup> Although the data shows frequent price changes, we focus on models of collusion on a stable price. In practice, practice firms in the retail petrol industry are likely to use more sophisticated strategies to coordinate tacitly on focal points (see Byrne and de Roos, 2019) for an empirical documentation of such a strategy). Other factors not included in the model may also affect whether collusion takes place, but a decrease in the critical discount factor makes it easier to collude. Changes in the critical discount factor should thus be seen as changes in the likelihood of collusion (similar to Cabral et al., 2019).

and Herre, 2013 and Petrikaite, 2016). This literature shows that slightly changing assumptions on the search protocol, the nature of goods or the elasticity of demand can have a great influence on whether increasing consumer information about prices is pro- or anti-competitive. Luco (2019) assumes firms to be imperfectly informed about the prices of their rivals, but does not allow consumers to search sequentially and only studies the case of differentiated goods. Schultz (2017) also assumes firms to be imperfectly informed and uninformed consumers not to search sequentially, but also studies homogeneous goods markets. He finds that under these conditions, increasing a common factor of transparency, affecting producers and consumers, is always anti-competitive in homogeneous goods markets. We derive a more general model for homogeneous goods by allowing consumers to search sequentially, as in Petrikaite (2016). In contrast to Schultz (2017) and Luco (2019), we find that the marginal effect of increasing transparency depends on the *ex ante* level of transparency and can also be pro-competitive in homogeneous goods markets.

Secondly, because mandatory price disclosure can have very different effects on prices theoretically, one might be worried about the external validity of the conclusions from a particular study. We therefore extend the empirical literature on the effect of mandatory price disclosure on prices. Albæk et al. (1997) and Luco (2019) find that increasing price transparency in homogeneous goods markets, namely the cement industry in Denmark and the petrol industry in Chile, led to an increase in prices. In contrast, we provide causal evidence of a mandatory price disclosure policy in a homogeneous goods market leading to a decrease in retail margins. The differences in results are in line with the predictions of the theoretical model given the policy design. In particular, two results of the theoretical model in this paper help explain the differences that we find: Firstly, if producers are well informed about prices before mandatory price disclosure, it is less likely that such a policy is anti-competitive. Secondly, in some cases a marginal increase in consumer transparency can be anti-competitive, whereas a large increase is pro-competitive. Thus, it is important that competition authorities do not only provide the information, but also push for its large-scale adoption.

Thirdly, we study how different market conditions affect the effect of mandatory price disclosure empirically and use these results to assess the suitability of different theoretical models. Ater and Rigbi (2018) show that newspapers play an important role in diffusing information from mandatory price disclosure for Israeli supermarkets. In contrast to their setting, we study a homogeneous goods market where the information adoption of price information by consumers is already high. We show, that even in this context follow-on local radio reports about petrol prices can lead to a further reduction in average prices. Next, we estimate heterogeneities in the treatment effect based on

the ex ante level of consumer transparency. We follow Pennerstorfer et al. (2019) in assuming that commuters are perfectly informed about petrol prices on their daily way to work to construct a proxy for the ex ante level of consumer transparency. Our empirical results are in line with predictions of the theoretical model in this paper and at odds with predictions of the theoretical models by Schultz (2017) and Luco (2019).<sup>9</sup> We thus conclude that the policy recommendations from the model in this paper are more suitable in this context than the recommendations from other candidate models.

Finally, the paper relates to the empirical literature on the optimal design of mandatory price disclosure policies. Martin (2018) uses price data from the MTU for petrol stations in Bavaria in 2017 to estimate a structural model and simulate the effect of limiting price transparency to only a few petrol stations. He finds that restricting transparency to showing only below-median prices, as opposed to full transparency, decreases consumer expenditures and increases consumer welfare. His findings are complementary to our study, as they highlight further possibilities to optimize the design of mandatory price disclosure policies.

The remainder of this paper is structured as follows: Section 4.2 describes the specificities of the German petrol market and the introduction of the MTU. Section 4.3 outlines the theoretical model. Section 4.4 presents the data used in this paper. Section 4.5 explains the empirical methodology and findings. Section 4.6 analyses potential mechanisms, Section 4.7 discusses the results and Section 4.8 concludes.

## 4.2 Background

Retail fuels are products with a very high degree of homogeneity within their product category. Although petrol filling stations also sell other products, we focus our attention on the sale of fuel. The two main fuel categories are diesel and petrol. Consumers cannot substitute between the two in the short-term, as vehicles can only either run on one or the other type. Within petrol, there is differentiation according to the octane rating and the share of ethanol. Even between these sub-categories, there is only very limited substitution. In our analysis, we, therefore, focus on petrol with an octane rating of 95 and an ethanol share of 5 percent, which had a market share of 82 percent in Germany in 2017 (Bundesverband der deutschen Bioethanolwirtschaft, 2018).

Examining the vertical structure is important to understand competitive dynamics in the petrol industry. In the upstream market, crude oil is directly transported to oil refineries, which then process the crude oil into retail products. These are sold

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<sup>9</sup> This does not mean that the theoretical model in this paper is more suitable in other contexts. However, the predictions of the different theoretical models allow assessing the suitability of the different models also in other contexts.



and distributed to the downstream market, where petrol filling stations sell the retail product to motorists.

According to the GFCO (2011), both the upstream and downstream markets are highly concentrated. The main firms active in the German upstream market are BP, ConocoPhillips, ENI, ExxonMobil, Rosneft, Shell and Total. In the downstream market, all of these firms with the exception of ENI and Rosneft also have a large network of petrol filling stations spread across Germany.<sup>10</sup> Access to refinery capacities is an important competitive advantage in the downstream market for petrol and therefore these five vertically integrated firms form an oligopoly with significant market power towards outside competitors, although there are also many other petrol filling stations.

In 2011, the five oligopolists had an aggregate market share of around 47 percent in retail petrol from petrol stations that they owned directly. However, this is an underestimate of the share of the market they control. A further 18 percent of the market is served by petrol stations that are owned and operated by independent dealers which usually operate under the brand of the oligopolists (see GMEAE, 2018). In its market investigation, the German Federal Cartel Office (GFCO) found that contractual relationships between the oligopolists and these independent dealers oftentimes allow the oligopolists to set prices for the independent dealers. It therefore seems plausible to consider all petrol stations belonging to one brand to act as an integrated firm.

The remaining 35 percent are made up of other vertically integrated competitors (mainly ENI, Orlen, OMV and Tamoil) and independently owned filling stations, which often buy their petrol from the oligopolists. Although independent stations not selling under the brand of the oligopolists can be considered less bound to pricing decision of the oligopoly, the GFCO found evidence that these are sometimes also constrained in their pricing decisions by contracts with the oligopolists.

Before the introduction of the MTU, price transparency among producers was high, but not perfect. Since the oligopolists have price discretion over their stations and, to some degree, about their partner petrol stations, they observed these prices before the MTU. Additionally, employees of company-owned stations, as well as dealer-owned partner stations, were often contractually bound to report prices of neighboring stations to the oligopolist several times a day (GFCO, 2011). Finally, some oligopolists also posted prices of their petrol stations on their website, which allowed other oligopoly members to observe these prices. However, many stations, in particular independent stations and small chains, did not follow the practice of posting prices online and only had limited knowledge of rivals' prices. In sum, there was considerable, but far from

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<sup>10</sup> ENI also has a small network of petrol filling stations in Germany, but its smaller size and reduced geographic reach gives it less clout.

perfect transparency on the producer side.

Before the MTU, consumers were much less informed about prices than firms and hence found it difficult to assess the competitiveness of a particular petrol price. In the absence of an information clearinghouse, there are significant search costs for consumers. To find the prices of all potential sellers, they would need to drive to all stations.<sup>11</sup>

A market investigation ending in 2011 led the GFCO to find that the oligopolists have a dominant position in regional petrol markets and that prices are higher than under functioning competition. Although the GFCO (2011) does not provide evidence of the existence of a hardcore cartel, there is significant evidence for tacitly collusive behavior. The GFCO found that a price increase at most stations of one of the oligopolists in a region usually starts a price cycle, which is an indication of collusive behavior. Aral and Shell are commonly found to be the price leader. Following a price increase, the other price leader (i.e. either Shell or Aral) reacts in 90% of cases by also increasing prices within 3 hours. This is usually followed by a price increase by Total, Jet or Esso only a few hours later. These price movements lead to price cycles with repeating patterns, which are primarily reflected in retail margins and thus not cost-driven. This is in line with findings by Byrne and de Roos (2019), who describe how dominant firms use price leadership and price experiments to create focal points that coordinate market prices, soften price competition and increase retail margins in the Australian petrol market.

After the market investigation, the GFCO and the German Monopolies Commission concluded that a lack of price transparency on the consumer side caused the lack of competition and therefore called for an increase in price transparency in the downstream market. In 2012, parliament passed a law which set out the creation of the market transparency unit under the management of the GFCO and on 12 September 2013 the operation of the MTU began. The MTU is an information clearinghouse that collects prices in real-time and allows app creators to diffuse the information to users. It hence provides consumers and producers access to live price data and increases price transparency on both sides of the market.

### 4.3 Theoretical Model

In this Section, we outline a theoretical model, which allows us to conceptualize how mandatory price disclosure affects collusionary behavior. We derive theoretical results

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<sup>11</sup> There were already some apps that allowed users to self-report petrol prices, which were then collected and diffused to users in a similar fashion to the price information from the MTU, but the usage of these apps before the MTU was considerably lower than after its introduction.

on the effect of increasing price transparency (e.g. through mandatory price disclosure) on the critical discount factor and thus the likelihood of collusive behavior.

In the model, a homogeneous good is sold by a small number of firms that could potentially engage in collusive behavior. At the same time, some consumers are perfectly informed about prices, whereas others are not.<sup>12</sup> Producers are also imperfectly informed about prices of their rivals, which means that it is possible for deviations from the collusionary agreement to remain undetected. These assumptions plausibly describe the competitive dynamics in the retail petrol market prior to the MTU and allow us to analyze how prices are affected when mandatory price disclosure affects the information sets of producers and consumers.

### 4.3.1 Setup

There is a finite number of  $n$  symmetric price-setting firms competing in an infinitely repeated game and producing at a normalized marginal cost of zero.

There is a unit-mass of consumers. Each consumer has the same valuation  $v$  for the good and inelastically demands  $\xi > 0$  units of the product.  $\xi$  is distributed according to the cumulative distribution function  $\psi(\xi)$  with mean equal to one. It is constant within a period but varies between periods. Firms thus do not perfectly know aggregate demand in a particular period.

A fraction  $\phi$  of consumers consists of fully informed shoppers and  $1 - \phi$  are non-shoppers, who can search sequentially. Shoppers know prices of all sellers and therefore always buy from the lowest price seller. Non-shoppers only know the distribution of prices and draw a first price for free. They can then choose to randomly draw prices of additional sellers at an incremental search cost  $s$ , in the hope of finding a lower price. Non-shoppers buy the good as soon as the price they draw is weakly below the reservation price  $p_r$ , at which non-shoppers are indifferent between accepting the price and drawing a new price at search cost  $s$ , because the expected price savings of drawing another price are equal to the search cost  $s$ .<sup>13</sup>

### 4.3.2 Static Equilibrium

In the static Nash equilibrium, all firms play mixed strategies in which they draw a price from the interval  $[p, p_r]$  according to the distribution  $G(p)$ , where  $p_r$  is the reservation price of non-shoppers and  $p$  is the minimum price a firm will charge. Shoppers always

<sup>12</sup>In modeling consumer information, our model builds on the collusion model for homogeneous goods markets proposed by Petrikaite (2016). However, in contrast to her model, we allow for imperfectly informed producers. She models consumer search as in the unit-demand version by Janssen et al. (2005) of the Stahl (1989) model.

<sup>13</sup>The model is solved in detail in Appendix D.1.

buy from the lowest price firm, whereas non-shoppers draw a single price and buy at this price. In equilibrium, non-shoppers hence do not search sequentially, because any price they draw is below their reservation price. In Appendix D.1, we derive expressions for the equilibrium objects  $p_r$ ,  $\underline{p}$ ,  $G(p)$  and  $\pi_i^*$ .

### 4.3.3 Dynamic Equilibrium

After analyzing the equilibrium of the one-shot game, we now look at an infinitely repeated version of this game, in which firms are allowed to collude and where deviations may remain undetected.

We assume that the price of a firm is observed by its rivals with probability  $\eta$ . Since aggregate demand is stochastic, firms cannot infer the prices of their rivals based on their own price and their quantities sold. A deviation from the collusionary agreement by a firm will therefore only be detected by its rivals with probability  $\eta$ .<sup>14</sup>

We assume that collusionary agreements are enforced by playing the grim-trigger strategy proposed by Friedman (1971). This means that if a deviation from the collusionary price is observed, all firms enter the punishment phase and charge the static equilibrium price forever.

A firm will charge the collusionary price as long as its expected discounted profit from collusion is at least as high as its expected discounted profit from deviation. This is the case if and only if:

$$\frac{1}{1-\delta}\pi^c \geq \pi^d + \frac{\delta}{1-\delta}(\eta\pi^* + (1-\eta)\pi^c), \quad (4.1)$$

where  $\delta$  is the discount factor,  $\pi^c$  is the expected profit under collusion,  $\pi^d$  is the expected deviation profit and  $\pi^*$  is the expected competitive profit in the static game.

In case a firm deviates from the collusionary agreement, it expects to receive the deviation profit  $\pi^d$  in the deviation period. In future periods, it expects to receive the competitive profits if its rivals detect its deviation and the collusionary profits if its deviation remains undetected.

To see what determines whether collusive behavior is profitable, we need to analyze the critical discount factor  $\underline{\delta}$ . If the discount factor of a firm is above  $\underline{\delta}$ , it values the future sufficiently to make a deviation, and thus the possibility of future punishment, unprofitable. If  $\delta < \underline{\delta}$ , a firm finds it profitable to deviate from the collusionary price. The critical discount factor is hence the discount factor at which Equation 4.1

<sup>14</sup>Note, that allowing collusion and imperfect information on the producer side has no effect in the static game, because in a one-shot game deviation is always profitable since it can never be punished in a subsequent period.

holds strictly and can be written as:

$$\underline{\delta} = \frac{\pi^d - \pi^c}{\pi^d - \eta\pi^* - (1 - \eta)\pi^c}. \quad (4.2)$$

The profit in punishment periods,  $\pi^*$ , corresponds to the competitive profits in the static game and the punishment price to the competitive price. In collusionary periods, firms charge the monopoly price, which, because consumers are perfectly inelastic, is equal to the valuation of the good by consumers, i.e.  $p_c = v$ . Since all firms charge the same price in collusionary periods, they all have an equal share of demand and the expected collusionary profits of a firm are  $\pi^C = \frac{v}{n}$ .<sup>15</sup> When a firm undercuts the collusionary price by charging  $p^d = v - \epsilon$ , where  $\epsilon$  is a very small positive number, it attracts all shoppers and keeps its share of non-shoppers. The expected deviation payoff is, therefore, approximately  $\pi^d = v(\frac{1-\phi}{n} + \phi)$ .

#### 4.3.4 Comparative Statics

Now that we know what determines prices, profits, and the incentives to collude, we can analyze how these are affected by changes in transparency and search costs.

Two special cases are worth analyzing. If  $\eta = 0$ , deviations from the collusionary agreement will never be detected. The critical discount factor to sustain collusion becomes 1. Firms will never adhere to the collusionary agreement, because a deviation is never detected and hence never punished. If  $\eta = 1$ , a deviation from the collusionary agreement will always be detected. In this case, mandatory price disclosure would only have an effect on the degree of price transparency on the consumer side. We will, therefore, analyze this special case in the context of a change in the share of shoppers,  $\phi$ . For the rest of the analysis, we focus on the case where  $\eta$  is strictly between zero and one.

**Proposition 1.** *A marginal increase in producer transparency,  $\eta$ , decreases the critical discount factor  $\underline{\delta}$ .*

If producer transparency increases, the likelihood that a rival detects a deviation from the collusionary agreement increases. Since the punishment phase starts only after detection of a deviation, an increase in producer transparency leads to an increase in the likelihood of punishment and thus makes deviation less attractive.<sup>16</sup>

**Proposition 2.** *A marginal increase in consumer transparency,  $\phi$ , weakly decreases*

<sup>15</sup>Sometimes, collusion on prices below the monopoly price is stable when collusion on the monopoly price is not. For simplicity, we restrict our analysis to collusion on the monopoly price.

<sup>16</sup>We prove this Proposition in Appendix D.2.

*the critical discount factor  $\underline{\delta}$  for small values of  $\phi$ ,  $\phi \leq \bar{\phi}$ , and increases the critical discount factor  $\underline{\delta}$  for values of  $\phi$ ,  $\phi \geq \bar{\phi}$ , for  $n = 2$ .*

Consumer transparency is modeled as the share of perfectly informed shoppers. If the market becomes more transparent for consumers, the share of shoppers increases. Analysing the effect of a change in  $\phi$  is more complicated than a change in  $\eta$  because  $\phi$  enters the deviation and punishment profits.

An increase in the share of shoppers,  $\phi$ , increases the number of consumers that would buy from a firm that marginally undercuts the collusive price and hence makes a deviation more attractive. To illustrate this point, let us focus on two extreme cases: If the share of shoppers is close to zero, then if  $\phi$  converges towards zero, the deviation profit converges towards the collusive profit. This is because if no consumer observes the deviation, then as many consumers randomly see the price of the producer and buy its good as under the collusive price. Producers thus have no incentive to deviate from the collusive agreement in the absence of informed shoppers and the critical discount factor becomes zero. If the share of shoppers is close to one, then if  $\phi$  converges towards one, the deviation profit converges towards the monopoly profit. This maximizes the incentive to deviate from the collusive agreement driven by the deviation profits.

At the same time, a higher share of shoppers decreases expected profits in the static Nash equilibrium, i.e. the punishment profit. This is because the higher the share of shoppers, the more consumers will go to the lowest price producer in a punishment phase. Furthermore, if the share of shoppers increases, the domain of prices over which firms mix moves towards lower prices.

The deviation profits increase linearly in the share of shoppers, whereas the punishment profits decrease in  $\phi$  but do this more strongly for small values of  $\phi$  than for larger ones. For very low values of  $\phi$ , the reservation price is higher than the valuation that consumers have for the good. In this case, the reservation price does not bind firm pricing and the critical discount factor does not change in case of a marginal increase in  $\phi$ . Overall, marginally increasing consumer transparency weakly decreases the critical discount factor  $\underline{\delta}$  for small values of  $\phi$ ,  $\phi \leq \bar{\phi}$ , and increases the critical discount factor  $\underline{\delta}$  for values of  $\phi$ ,  $\phi \geq \bar{\phi}$ .<sup>17</sup>

So far, we treated consumer and producer transparency as dichotomous. In reality, unless producers are already perfectly informed ex ante, an increase in consumer transparency often also comes with an increase in transparency on the producer side. Therefore, we introduce a common factor of transparency  $\alpha$  such that  $\phi = \phi(\alpha)$  with  $\phi'(\alpha) > 0$  and  $\eta = \eta(\alpha)$  with  $\eta'(\alpha) > 0$ . Before analyzing the effect of a change in

<sup>17</sup> We prove this for  $n = 2$  in Appendix D.2 and illustrate this using a numerical example in Figure 4.1. Petrikaite (2016) shows numerically that this is also the case for  $n = 3$ ,  $n = 4$  and  $n = 6$ .

$\alpha$  on the profitability of engaging in collusive behavior, it is helpful to introduce the elasticity of  $\eta$  with respect to  $\alpha$ :

$$e_{\eta,\alpha} \equiv \frac{\partial \eta}{\partial \alpha} \frac{\alpha}{\eta}, \quad (4.3)$$

and the elasticity of  $\phi$  with respect to  $\alpha$ :

$$e_{\phi,\alpha} \equiv \frac{\partial \phi}{\partial \alpha} \frac{\alpha}{\phi}. \quad (4.4)$$

**Proposition 3.** *A marginal increase in common transparency  $\alpha$  can increase or decrease the critical discount factor  $\bar{\delta}$ , depending on the ex ante levels of transparency  $\eta$  and  $\phi$ , as well as the elasticities of  $\eta$  and  $\phi$  with respect to  $\alpha$ .*

If a common factor of transparency  $\alpha$  increases, for example through mandatory price disclosure, then  $\eta$  and  $\phi$  both increase so long as neither producers nor consumers are perfectly informed ex ante. To understand how the marginal increase of a common factor of transparency affects the critical discount factor, we must thus analyze its effect through the channels described in Propositions 1 and 2. We do this separately for the two cases introduced in Proposition 2.

For low values of consumer transparency  $\phi$ ,  $\phi \leq \bar{\phi}$ , a marginal increase in  $\phi$  decreases the critical discount factor. At the same time, a marginal increase in producer transparency decreases the critical discount factor. Thus, for low values of  $\phi$ ,  $\phi \leq \bar{\phi}$ , a marginal increase in common transparency  $\alpha$  always decreases the critical discount factor and is hence anti-competitive.

For high values of consumer transparency  $\phi$ ,  $\phi \geq \bar{\phi}$ , a marginal increase in  $\phi$  increases the critical discount factor. However, a marginal increase in producer transparency continues to decrease the critical discount factor. In this case, whether a marginal increase in a common factor of transparency is pro- or anti-competitive thus depends on the relative size of these two countervailing effects. In Appendix D.1, we derive conditions under which increasing common transparency makes collusive behavior less stable and is thus pro-competitive.

#### 4.3.5 Theoretical Predictions

There are two other types of theoretical models that allow for changes in producer and consumer transparency at the same time. Firstly, Schultz (2017) and Luco (2019) show that for differentiated products increasing a common factor of price transparency can be pro- or anti-competitive. Secondly, Schultz (2017) shows that for homogeneous goods, increasing a common factor of price transparency is always anti-competitive

and increasing consumer transparency has no effect on the critical discount factor if producers are already perfectly informed. Our results are thus in strong contrast to previous findings in the theoretical literature, as we are, to the best of our knowledge, the first to show that increasing common transparency can increase the critical discount factor in homogeneous goods markets.<sup>18</sup> This difference in results is caused by our assumption to allow non-shoppers to search sequentially. Our results have important policy implications because if increasing price transparency on both sides of the market was always anti-competitive, such a policy should never be considered by a consumer-centric policymaker.

At the same time, Proposition 2 shows that even if producers are perfectly informed ex ante (i.e.  $\eta = 1$ ), a marginal increase in consumer transparency can be pro- or anti-competitive. Thus, even if firms are perfectly informed ex ante, the direction of the effect of mandatory price disclosure on prices remains an empirical question.

The predicted effect of a marginal increase in consumer transparency is very different depending on the ex ante levels of transparency. In Figure 4.1, we illustrate how a change in transparency affects the critical discount factor for a given set of parameter values.<sup>19</sup> Note, that when the reservation price, which depends on the sequential search cost and the share of informed consumers, is greater than the valuation of the good by consumers, the reservation price does not restrict pricing of the firms anymore. In this case, the critical discount factor is independent of the share of informed consumers.

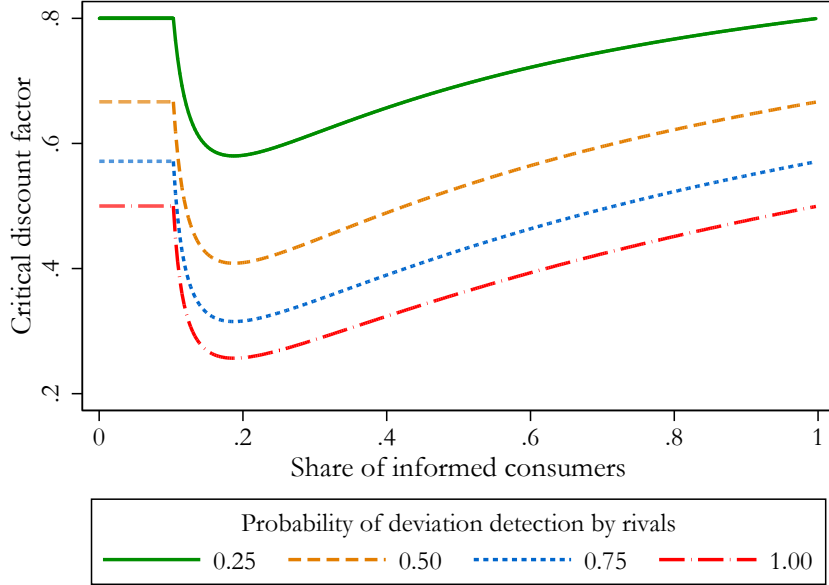
Figure 4.1 shows the relationship between consumer transparency and the critical discount factor for different levels of producer transparency. An increase in producer transparency leads to a shift in the curve and always results in a decrease of the critical discount factor. If mandatory price disclosure affects consumer and producer transparency, this therefore leads to a shift of the curve (increase in producer transparency), as well as a move along the curve (increase in consumer transparency). Depending on the ex ante levels of consumer and producer transparency, as well as the magnitude of the change in consumer and producer transparency, mandatory price disclosure can thus increase or decrease the critical discount factor.

Some of these predicted effects are in line with predictions from other models, whereas others are not. To contrast predictions of the different theoretical models, we focus on the effect of changes in consumer transparency. For very low levels of consumer transparency, a marginal increase in  $\phi$  does not affect the critical discount

<sup>18</sup> Petrikaite (2016) shows that increasing consumer transparency can be pro- or anti-competitive in homogeneous goods markets and we extend her model by allowing producers to be imperfectly informed and analyzing changes in common transparency.

<sup>19</sup> We set the number of firms to  $n = 2$ , the sequential search cost  $s = 1$  and the valuation of the good  $v = 10$ , according to the values chosen by Petrikaite (2016) in her simulations. The pattern looks similar for other parameter values.



**Figure 4.1:** Effect of transparency on the critical discount factor

Notes: Each line shows how the critical discount factor changes with the level of consumer transparency for a given level of producer transparency. The different lines show this relationship for different levels of producer transparency. All simulations use the following parameter values:  $n = 2$ ,  $s = 1$  and  $v = 10$ .

factor. This is observationally equivalent to how a change in consumer transparency affects the critical discount factor in the homogeneous goods models of Schultz (2017).

When  $\phi$  is sufficiently high such that the reservation price is below the valuation of the good by consumers, but  $\phi \leq \bar{\phi}$ , a marginal increase in  $\phi$  decreases the critical discount factor. Observing such a pattern is never consistent with the models in Schultz (2017) or Luco (2019).

For intermediate levels of  $\phi$ , a marginal increase in consumer transparency increases the critical discount factor and the absolute value of the marginal effect increases in the ex ante level of consumer transparency. Observing such a pattern is never consistent with the models in Schultz (2017) or Luco (2019).

For high levels of  $\phi$ , a marginal increase in consumer transparency increases the critical discount factor and the absolute value of the marginal effect decreases. This is observationally equivalent to the effect of an increase in consumer transparency in the differentiated goods models in Schultz (2017) or Luco (2019).

In the model described in this paper, for intermediate levels of consumer transparency a marginal increase in  $\phi$  thus leads to an effect on the critical discount factor which is inconsistent with other candidate models. Empirical evidence may hence allow to assess which model best fits a particular market.

Finally, an interesting result of the theoretical model is the U-shaped relationship between consumer information and the profitability of collusive behavior. This implies that, under certain circumstances, it is possible that a marginal increase in consumer information is anti-competitive, whereas a large increase is pro-competitive. However, if a marginal increase in consumer information is pro-competitive, then a large increase is always pro-competitive. Therefore, if a competition authority finds that the market is in a situation where a marginal increase in consumer information induces collusive behavior, it should nevertheless assess how large of an increase in consumer information would be necessary to make it pro-competitive and whether this is feasible.

## 4.4 Data

To analyze the effect of mandatory price disclosure, we combine data from multiple sources. Our core data set contains station-level retail margins for the universe of petrol stations in Germany and France for the years 2013 and 2014. We supplement this with information on local media reports about local petrol prices by radio stations and a proxy for the station-level share of ex ante perfectly informed consumers based on commuter data.

### 4.4.1 Retail Margins and Petrol Station Characteristics

We construct our primary data set containing station-level retail margins for *E5* petrol on weekdays at 9 am and 5 pm between 1 January 2013 and 31 December 2014 in Germany.<sup>20</sup> To calculate retail margins, we subtract the daily average ex-refinery wholesale price across refineries in Germany from the station-level retail petrol prices. Station-level retail margins are complemented by station characteristics such as information on the firm name, brand, address and geographic coordinates.

The price panel after the MTU introduction is constructed based on all station-level notifications of price changes to the MTU since 1 October 2013. Each time a new price for a station is notified, we update the price of the station in our panel data set.

A novel and unique feature of our data is that, to the best of our knowledge, we are the first to use rich station-level price information *before* the introduction of the MTU. Before the introduction of the MTU, some smartphone apps existed that allowed their users to self-report station-level petrol prices. Although the usage of these apps was only a fraction of the usage of price comparison apps after the MTU introduction and the publicity that came with it, the pre-MTU apps contain rich price

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<sup>20</sup> *E5* is standard petrol with an octane grade of 95 and an ethanol share of up to 5%.

information.<sup>21</sup> We use price data for the pre-MTU period supplied by one of the leading apps collecting self-reported prices. This data set comprises 17 million price reports for more than 13,500 stations between 1 January and 12 September 2013. Although the MTU went into operation on 12 September 2013, we only have access to its data from the 1 October 2013 onwards. Since our self-reported pre-MTU data only goes until the 12 September 2013, the period in between is not subject of our analysis.

For most days in the pre-MTU period, we have prices for more than 80% of petrol stations.<sup>22</sup> In case the reporting of prices is not random, selection could harm the validity of our estimation results. The most natural selection mechanism is that petrol stations themselves report prices onto the apps when they are low to attract shoppers. At the same time, they could refrain from posting prices when they are high in order not to discourage consumers from driving to their petrol station and discover the price. In this case, prices in our sample before the MTU introduction should be downward-biased. However, since we find that prices decreased after the introduction of the MTU, this selection mechanism would work against us, and our estimates can be seen as a lower bound.

Another concern could be that the composition of petrol stations changed in our sample before and after the introduction of the MTU. Table 4.1 presents summary statistics of our data. As can be seen in Panel A, the composition of petrol stations in our data does not change significantly between the pre- and post-MTU periods concerning the share of integrated stations, the share of oligopoly stations, the share of commuters or the number of competitors in local petrol markets. A detailed split of petrol stations by brand before and after the MTU introduction can be found in Table D.1 of Appendix D.3. Overall, the composition of brands is very similar.<sup>23</sup>

The largest share of the retail price for petrol in Germany consists of taxes and input costs. To analyze the share of the petrol price that can be influenced by petrol stations, we focus on retail margins. Firstly, we subtract taxes and levies to compute net petrol prices. Thereafter, we subtract the daily average ex-refinery price of *E5* at German oil refineries to obtain retail margins. Daily ex-refinery prices are taken from

<sup>21</sup> Figure D.7 shows for three mobile price comparison applications for which data is available in 2014, that the number of page impressions increased strongly over the course of the year 2014.

<sup>22</sup> The daily number of petrol stations with price reports and the number of daily price changes are reported in Figures D.2 and D.3. We exclude days after the MTU introduction from our analysis, where the number of price changes compared to the previous day drop by more than 40%. Since we observe the universe of price changes after the introduction of the MTU, and the average number of daily price changes is usually stable, we conclude that these days are affected by technical difficulties. In total, this affects ten days during the 15 months of data used from the MTU.

<sup>23</sup> To exclude the possibility that small changes in the brand composition drive our results, we repeat the analysis for different sub-samples such as integrated and non-integrated stations, as well as for the two largest brands, Aral and Shell, in Appendix D.11. These additional analyses show that our results are robust to changes in the sample.

**Table 4.1:** Summary statistics

A. Station characteristics					
	DE pre-MTU	DE post-MTU	France		
Number of stations	13,704	14,414	7,240		
Share of integrated stations	59%	58%			
Share of oligopoly stations	48%	47%			
Median # comp. (5 km catchments)	4	3	2		
Share of local monopolists	15%	15%	22%		
Mean share of commuters	34%	34%			
B. Prices and margins					
	DE pre-MTU at 9 am	DE post-MTU at 9 am	DE pre-MTU at 5 pm	DE post-MTU at 5 pm	France at 5 pm
Mean price	1.62	1.55	1.59	1.50	1.54
Mean retail margin	0.10	0.11	0.08	0.06	0.19
Mean daily spread	0.09	0.09	0.09	0.07	0.15

Notes: “DE pre-MTU” and “DE post-MTU” refer to petrol stations in Germany before and after the introduction of the MTU, respectively. The pre-MTU phase goes from 1 January 2013 until 12 September 2013. The post-MTU phase goes from 1 October 2013 until 31 December 2014. For France, all figures are for the full period 1 January 2013 until 31 December 2014. The average daily spread is measured as the average of the difference between the retail margin at the 95<sup>th</sup> percentile and the 5<sup>th</sup> on each day.

Oil Market Reports, a business intelligence firm, generally regarded as the most reliable source for refinery prices and used as a data source by the GFCO.

Since January 2007, all petrol stations in France selling more than 500m<sup>3</sup> of petrol per year have to report all price changes to a government agency similar to the MTU in Germany. Regular checks are carried out and fines imposed on petrol stations that do not comply with this rule. To the best of our knowledge, France is the only other European country for which station-level petrol prices are available during this period.<sup>24</sup> The French government makes all price information since 2007 publicly available on a government website.<sup>25</sup> We thus observe the universe of price changes of these petrol stations in France for our observation period. The data is regarded to be of very high quality and has previously been used by other researchers.<sup>26</sup>

The data set contains a list of notifications with the price, the type of fuel, the address and geographic coordinates of the petrol stations and the opening times. In

<sup>24</sup> Austria introduced mandatory price disclosure in 2011, however only published the five lowest prices in a local market. In addition, daily average prices at the state level are available for Austria. We, therefore, show descriptive results of the effect of the MTU introduction using Austria as a control group in Appendix D.8. These results are consistent with our main results.

<sup>25</sup> <https://www.prix-carburants.gouv.fr/rubrique/opendata/>. Accessed 3 February 2019.

<sup>26</sup> Gautier and Saout (2015), for example, use this data to study the speed at which market prices of refined oil are transmitted to retail petrol prices.

contrast to the data of the MTU in Germany, it does not contain any information on the brand of the station or any other company-related information.

To compute retail margins, we also need a measure for input prices in France. To the best of our knowledge, there is no comparable data to the ex-refinery prices for Germany by Oil Market Reports available for France. We thus use daily market prices for refined oil at the port of Rotterdam as a proxy for ex-refinery prices in France.

#### 4.4.2 Exogenous Information Shocks

To study the effect of a follow-on information campaign, we analyze the impact of local radio reports about petrol prices in the radio station's reception area. The creation of the MTU made it easy for local radio stations to access real-time information on the distribution of petrol prices in their reception area. Although the MTU introduction changed the information sets of consumers, this does not mean that all consumers suddenly knew all prices. Follow-on radio reports thus constitute additional shocks to consumer information. At the same time, it seems plausible that petrol stations immediately incorporated the price information by the MTU and hence were unaffected by additional radio reports. We, therefore, consider these reports to be pure shocks to  $\phi$  in our theoretical model.

To study the impact of local radio reports, we hand-collected data on regular broadcasts of local petrol prices by local radio stations in Bavaria, the largest region in Germany. By contacting program directors of around 60 radio stations, we collected information on which radio stations broadcasted petrol prices at which point in time between 2012 and 2017. There are two local radio stations regularly broadcasting petrol prices after the introduction of the MTU. *Radio Arabella*, which started its broadcast on 25 April 2014, and *Extra-Radio*, which started its broadcasts on 2 February 2014. Using geocoded reception areas for these radio stations provided by *fm-list.org* and combining this with the geocoded location of petrol stations, we know for which petrol station potential customers were affected by these broadcasts on which day.<sup>27</sup>

#### 4.4.3 Consumer Search and Information

The theoretical model suggests that the effect of an increase in transparency depends on the ex ante share of informed consumers. We follow Pennerstorfer et al. (2019) to compute the share of ex ante informed consumers at the petrol station level. Out-of-municipality commuters are assumed to be perfectly informed about petrol stations which they drive past on their daily commute. Drivers that do not leave their muni-

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<sup>27</sup> A detailed description of the radio station reports can be found in Appendix D.5.

cipality on their way to work are uninformed about prices and have to search. The share of ex ante informed consumers is thus the share of commuters among potential customers of a station.

To calculate this, we use administrative data on the universe of commuters, collected by the German Federal Employment Agency. The data includes the number of commuters from one municipality to another for all 11,197 municipalities in Germany. To calculate how many out-of-municipality commuters drive by a particular petrol station, we use a shortest-path algorithm to calculate the driving distance between two municipality centroids via the road network. We then calculate the driving distance between the two municipality centroids and a petrol station. If the sum of the distances between the municipality centroids and the petrol station are less than 250 meters longer than the shortest path between the two centroids, we assume that commuters on this route drive by the particular petrol station. Since we are interested in the ex ante level of information, we use commuter data on 30 June 2013, which is less than three months before the introduction of the MTU.<sup>28</sup>

## 4.5 Empirical Analysis

We begin by studying the average effect of mandatory price disclosure. We do this by comparing the evolution of retail margins of petrol stations in Germany and France before and after the introduction of the MTU. In terms of the theoretical model, the MTU introduction is a shock to  $\alpha$ , since both, producers and consumers, are affected. In Section 4.6 we disentangle the mechanisms underlying this average effect.

### 4.5.1 Setup

The introduction of the MTU in Germany led to the creation of a large number of new websites and smartphone applications diffusing petrol price information and was accompanied by numerous reports in the press. In the months following the introduction of the MTU, the number of drivers using price information services strongly increased, and petrol station managers universally had easy access to real-time price information of their rivals.<sup>29</sup>

To causally study the average effect of the MTU on retail margins, we use a difference-in-differences (DiD) framework in which we compare retail margins of petrol

<sup>28</sup> The methodology to compute the share of ex ante informed consumers is described in Appendix D.6.

<sup>29</sup> We present evidence on the evolution of monthly page impressions for three smartphone applications in Appendix D.3. Although the applications already had a combined number of 9 million monthly page impressions in April 2014, this increased to more than 70 million monthly page impressions in December 2014. Monthly page impression data for the smartphone applications is only available starting in April 2014.

stations in Germany to those in France, before and after the introduction of the MTU. Specifically, we estimate the following fixed effects regression:

$$Y_{it} = \beta_0 + \beta_1 MTU_{it} + \mu_i + \gamma_t + \epsilon_{it} \quad (4.5)$$

where  $Y_{it}$  corresponds to the retail margin of station  $i$  at date  $t$  and  $MTU_{it}$  is a dummy equal to one, if petrol station  $i$  has to report its prices to the MTU at date  $t$ . This affects all petrol stations in Germany after the 1 October 2013.  $\mu_i$  are petrol station fixed effects, and  $\gamma_t$  are date fixed effects.

### France as a Control Group

To identify the causal effect of the introduction of the MTU, we use the evolution of retail margins at petrol stations in France as a comparison. To the best of our knowledge, France is the only other country for which station-level petrol prices and retail margins are available for most stations for the full observation period.

Two assumptions need to be met to identify the causal effect of the MTU in our framework: The first assumption is that there cannot be any other transitory shocks affecting petrol stations in France and Germany differently before and after the introduction of the MTU other than the introduction of the MTU itself. The second assumption is that there are no spillovers from the treatment onto the control group. Subsequently, we provide evidence that suggests that both assumptions hold.

The station fixed effects capture time-invariant differences between petrol stations in France and Germany. The date fixed effects capture transitory shocks that affect French and German petrol stations equally. Due to its similarities in size, wealth and geographic location, as well as our narrow observation period, there should not be any additional transitory demand and supply shocks that affect France and Germany differently. We nevertheless discuss the most obvious candidates.

Important transitory demand shocks in the petrol market are school and public holidays, as well as local economic shocks. School and public holidays in France and Germany are highly correlated. In addition, since holidaymakers in Europe often cross several countries on the way to their holiday destination and France and Germany are popular holiday destinations and important transit countries, they are usually hit similarly and at the same time by these demand shocks. A second concern could be local economic shocks affecting cargo shipping and thus the demand for petrol. Since commercial vehicles, such as trucks transporting cargo and company cars usually run on diesel, our analysis of the effects of the MTU on prices of *E5* (i.e. regular petrol) should be unaffected by transitory local shocks to the economy.

Transitory supply shocks affect petrol stations much in the same way. Due to their geographic proximity, petrol stations in France and Germany procure most of their petrol from similar sources. Furthermore, the European Single Market and the Schengen Agreement mean customs, border controls or other regulatory hurdles do not restrict arbitrage possibilities between the two countries. To nevertheless ensure the elimination of any transitory shocks to input prices and to restrict our analysis to the share of the petrol price that can be affected by petrol stations, we use retail margins as outcome variables instead of prices. These retail margins are net of taxes, levies and ex-refinery petrol prices on a given day.<sup>30</sup>

Also, petrol stations in France constitute a good control group because there were no important regulatory changes in the French petrol market over our observation period. The impact of the introduction of mandatory price disclosure in 2007 should have stabilized by 2013 and thus not affect different French petrol stations differently over our observation period. In contrast to other countries, France, like Germany, did not restrict its petrol stations in their price-setting behavior other than by imposing mandatory price disclosure.<sup>31</sup>

One might be worried that there may still be idiosyncratic developments, which add random noise to the data and thus lead to an underestimation of the absolute value of the effects. We, therefore, re-run our analysis for a subsample of the data around the Franco-German border, for which the economic conditions should be similar due to geographic proximity. Firstly, we restrict our analysis to petrol stations that are 40 kilometers left and right to the border. Petrol stations in the treatment and control groups are thus in the same economic area and only exposed to common transitory shocks. Secondly, to eliminate any potential spillover effects, we drop all petrol stations that are less than 20 kilometers left and right of the border. We are left with a Donut-DiD, where stations on both sides of the border are geographically close, but stations that are potentially subject to spillover effects are dropped.

Finally, we re-run our analysis comparing retail margins in Germany and France after excluding local monopolists. If mandatory price disclosure has an effect on retail margins, we expect the size of the treatment effect to be larger after excluding local monopolists, because their customers are unaffected by the information treatment, since they have no competitor to switch to. Driving to another petrol station is costly and hence retail petrol markets are usually segmented geographically. In the following,

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<sup>30</sup> Since we do not have ex-refinery prices for France, we use the wholesale price of Brent oil in Rotterdam as a proxy. The station fixed effects should capture time-invariant differences between ex-refinery prices and the wholesale price in Rotterdam.

<sup>31</sup> In 2011, Austria, for example, introduced a rule banning petrol stations from raising prices more than once a day.



we define local markets as 5 kilometers driving distance catchment areas around a focal station.<sup>32</sup>

In the appendix, we report results of using further identification strategies to test the robustness of our estimates. These include comparing retail margins between Germany and Austria, analyzing differences between local monopolists and other petrol stations in Germany, and using a triple-difference strategy, in which we compare local monopolists to other petrol stations, in Germany and France, before and after the introduction of the MTU. All of our findings are in line with our main findings and suggest that our estimated average treatment effect is robust.

### Retail Margins at 5 pm

Although there are no restrictions on the number of times petrol stations can change prices in France or Germany, there are strong differences in the number of times they do. Whereas petrol stations in Germany change their prices on average four times a day over our observation period, French petrol stations change prices less than once a day.<sup>33</sup> Since we do not observe volume data, we cannot compute volume-weighted average retail margins over the day. We could thus either pick a particular time of day at which to measure retail margins or calculate a simple average of margins at different times of the day. Since petrol prices in France stay fairly constant during the day, either approach should lead to a similar result for France. The frequent price changes in Germany however, make it important to select the right time for which to calculate retail margins.

We choose to use prices at 5 pm to construct retail margins for our analysis. A representative survey among motorists commissioned by the GMEAE (2018) in 2016 found that around 60 percent of respondents buy petrol between 4 pm and 7 pm, of which two-thirds buy petrol between 5 pm and 6 pm. At the same time, less than 5 percent of respondents buy petrol before 10 am.<sup>34</sup> The GMEAE (2018) furthermore documents daily price cycles with high prices in the morning, which fall over the day and rise again in the evening at around 8 pm.<sup>35</sup> This suggests that consumers are aware of these price cycles and fuel during the low price period in the late afternoon.<sup>36</sup>

<sup>32</sup>The empirical literature analyzing price dispersion in retail petrol markets considers different geographic market definitions. For example, Chandra and Tappata (2011) consider a 1 mile as well as a 2 miles radius, while Barron et al. (2004), Hosken et al. (2008) and Lewis (2008) consider a radius of 1.5 miles. We use different catchment sizes in further results in Appendix D.9.

<sup>33</sup>This is consistent with findings by Haucap et al. (2017) for Germany and Gautier and Saout (2015) for France.

<sup>34</sup>The daily fueling patterns are described in detail in Figure D.5 in Appendix D.3.

<sup>35</sup>This is consistent with pricing patterns in the data described in Figure D.6 in Appendix D.3.

<sup>36</sup>There are numerous newspaper articles on intertemporal price dispersion during our observation period, which suggest that consumers are aware of these patterns.

To gauge the effect of introducing mandatory price disclosure on consumers, it is therefore sensible to focus on retail margins at times where consumers buy petrol in large volumes.

#### 4.5.2 Results

Figure 4.2 shows the evolution of indexed retail margins in France and Germany between April 2013 and December 2014. Since the MTU was introduced on the 12 September 2013, our observation period begins 5 months before its introduction.<sup>37</sup> At first, the MTU was in a test phase, which lasted until 1 December 2013, after which the MTU was fully launched. The beginning of the test phase is represented by the solid line and the beginning of the full-scale phase by the dashed line. We index the retail margins in France and Germany by their level on the first day of our observation period, to account for differences in the levels of retail margins.<sup>38</sup>

We see that before the introduction of mandatory price disclosure in Germany, the relative evolution of retail margins in both countries was similar. The parallel evolution of retail margins continues until December 2013, after which indexed retail margins in Germany fall significantly below indexed retail margins in France. This difference remains until the end of our observation period. It suggests that the effect of the MTU did not set in immediately, but caused a persistent decrease in retail margins. In particular, indexed retail margins only separate substantially after the beginning of the full-scale phase of the MTU.<sup>39</sup>

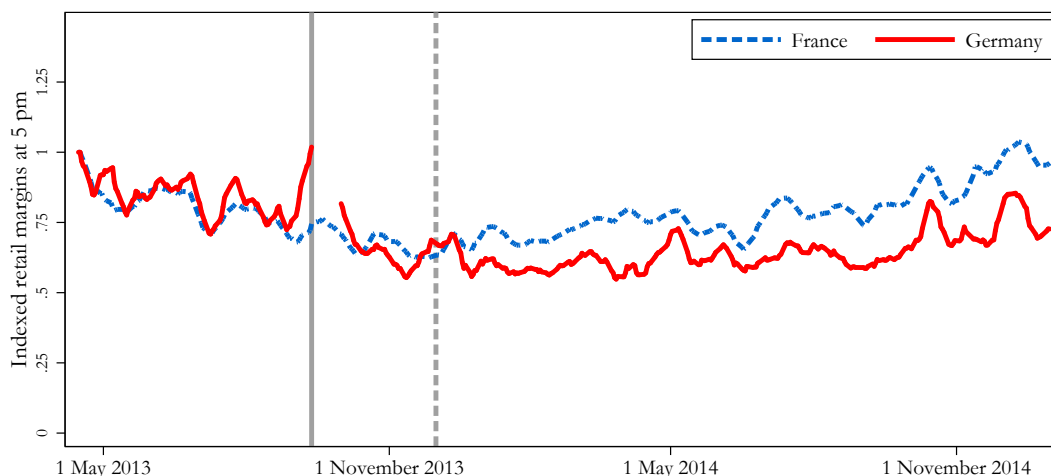
Between 12 September and 1 December 2013, the MTU was in a test phase. During this time, petrol stations, the competition authority and information providers diffusing the price information were experimenting with the technical implementation. We therefore exclude the test phase from our analysis and compare retail margins before the beginning of the test phase to after the MTU was fully implemented.

Table 4.2 shows the results of estimating the regression model presented in Equa-

<sup>37</sup>Including March 2013 would significantly add noise to our estimations. There is a large, short-term drop in retail margins in Germany at the beginning of March, which increases and a reversion to previous levels during the second half of the month. This is not an artifact of our data, since the same spike can be seen in the Weekly Oil Bulletin by the European Commission for Germany only, but cannot be observed in ex-refinery prices.

<sup>38</sup>Note, that we are using ex-refinery prices as input costs for Germany (i.e. after transportation to the refinery and refining), whereas we use the wholesale Brent oil price as input costs for France (i.e. without transportation and refining costs). We would therefore expect estimated margins to be higher for France, which is what we observe.

<sup>39</sup>There is a strong temporary increase in relative retail margins shortly before the introduction of the MTU in September 2013. To show that our results are not driven by this temporary increase in Germany relative to France, we re-estimate our main results dropping September 2013. The results of this analysis are presented in Table D.7 in Appendix D.13 and are in line with our main results in Table 4.2.

**Figure 4.2:** Evolution of retail margins

Notes: The solid line shows the evolution of the eleven-day moving average indexed daily average retail margin of petrol stations at 5 pm in Germany between April 2013 and December 2014. The dashed line shows the evolution of the eleven-day moving average indexed daily average retail margin of petrol stations in France at 5 pm over the same period. The average retail margins at the beginning of the observation period, on 12 April 2013, are indexed to one. The solid vertical line shows the beginning of the test phase of the MTU, on 12 September 2013. The dashed vertical line shows the beginning of the full-scale phase of the MTU, on 1 December 2013.

tion 4.5. To allow for the treatment effect to take some time to set in, we estimate the model for two different lengths in time after the introduction of the MTU. Column (1) shows the estimates using data until 30 September 2014 and column (2) until 31 December 2014. In columns (3) and (4), we re-estimate the model for the different time-periods only using a sub-sample of petrol stations 20 to 40 kilometers away from the Franco-German border. In columns (5) and (6), we re-estimate the model for the different time-periods using all petrol stations in France except for local monopolists defined on 5 kilometers catchment areas.

The results in columns (1) and (2) show that the MTU led to a statistically and economically significant decrease in retail margins and that this effect grew over time. Considering data until September 2014, we find that the MTU decreased retail margins by 0.89 Eurocent. This effect increases to 1.63 Eurocent if we consider the period until December 2014.

These results suggest a slow onset of the effect of the MTU, which becomes stronger over time. The findings are in line with evidence on the evolution of monthly page impressions for three smartphone applications presented in Appendix D.3. Although the applications already had a combined number of 9 million monthly page impressions in April 2014, this increased to more than 70 million monthly page impres-

**Table 4.2:** The effect of the MTU on retail margins

	(1)	(2)	(3)	(4)	(5)	(6)
MTU introduction	-0.89*** (0.02)	-1.63*** (0.03)	-1.18*** (0.18)	-1.88*** (0.23)	-0.97*** (0.02)	-1.74*** (0.03)
Date fixed effects	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Station fixed effects	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Observations	6,219,476	7,573,649	111,319	136,561	5,196,002	6,323,512
Adjusted $R^2$	0.921	0.904	0.904	0.886	0.919	0.902
Mean retail margin	10.90	11.15	10.42	10.71	10.56	10.80

Notes: The test phase of the MTU (12 September 2013 until 30 November 2013) is dropped in all specifications. Column (1) uses data for all stations in Germany and France until 30 September 2014. Column (2) uses data for all stations in Germany and France until 31 December 2014. Column (3) uses only data for stations 20 to 40 km away from the Franco-German border until 30 September 2014. Column (4) uses only data for stations 20 to 40 km away from the Franco-German border until 31 December 2014. Column (5) uses data for all petrol stations except those without a competitor within a 5 km catchment area in Germany and France until 30 September 2014. Column (6) uses data for all petrol stations except those without a competitor within a 5 km catchment area in Germany and France until 31 December 2014.

Standard errors, clustered at the petrol station level, are in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

sions in December 2014.<sup>40</sup>

Columns (3) and (4) show the results for the Donut-DiD regression, using only the sub-sample of petrol stations 20 to 40 kilometers away from the Franco-German border.<sup>41</sup> As expected, the absolute values of the coefficients are larger than those in columns (1) and (2). As explained before, this is likely due to idiosyncratic changes in retail margins, which are independent of the MTU and the treatment and control groups, but add random noise to the data and thus lead to an underestimation of the absolute value of the treatment effect.

Finally, when comparing results in columns (5) and (6) to the baseline results in columns (1) and (2), we find that if we exclude local monopolists on both sides of the border from the analysis, this increases the size of the magnitude. This is consistent with the treatment effect being attributed to mandatory price disclosure because we expect its effect to be zero on local monopolists and thus including these in the treatment group should bias the results towards zero.

Overall, the results suggest that mandatory price disclosure led to a significant and persistent decrease in average retail margins in Germany. Considering that retail

<sup>40</sup> Monthly page impression data for the smartphone applications is only available starting in April 2014.

<sup>41</sup> The results are robust to changes to the distance thresholds. We provide estimates for alternative thresholds in Appendix D.7.

margins in Germany were 7.9 Eurocent on average during our observation period, the estimated effects of the MTU are economically significant. With a treatment effect of approximately 1 Eurocent, the MTU led to a decrease in retail margins by 13 percent.

## 4.6 Mechanisms

A key question is what determines whether mandatory price disclosure is beneficial for consumers or not. The theoretical model in Section 4.3 showed that two factors drive the effect of mandatory price disclosure: The number of consumers adopting the information from mandatory price disclosure and the share of consumers that are already informed about prices before the information shock. In the following, we provide evidence on these mechanisms.

### 4.6.1 Effect of a Follow-on Shock to Consumer Transparency

Using local radio reports about petrol prices, we study empirically how follow-on information campaigns affect retail margins. A few months after mandatory price disclosure was introduced, some local radio stations started broadcasting the lowest petrol prices in their reception area. Although only some consumers adopted the information of the MTU directly, it is reasonable to assume that producers immediately adopted the available price information. Thus, local radio reports affect the information sets of consumers, but not producers and can be considered as pure shocks to consumer transparency,  $\phi$ .

Petrol prices are an often discussed topic amongst drivers, of which many listen to local radio stations on their daily commute. Petrol prices have therefore always been a recurring segment for local radio stations, but only after the introduction of the MTU did they have a tool to view the distribution of petrol prices in their reception area at low cost. Some of these radio stations, therefore, started diffusing the cheapest petrol prices in their reception area and their respective prices several times a day. Although for some listeners the listed stations might not be sufficiently close by, the broadcasts nevertheless inform them about the current lower end of the price distribution in their region.

To estimate the effect of local radio reports on petrol prices, we collected information for all radio stations in Bavaria whether they started regular broadcasts of petrol prices after the introduction of the MTU and which petrol stations lie within their reception area.<sup>42</sup> We identify four radio stations that reported the lowest petrol prices in their reception area regularly. Two of these radio stations allowed listeners

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<sup>42</sup>We define “regularly” as at least once a day over a period of more than a month.

to call in petrol prices before the MTU and then reported the lowest called in prices. We exclude all petrol stations in their reception areas from the analysis, as they are treated throughout the observation period. Using a DiD framework, we estimate the following fixed effects regression model:

$$Y_{it} = \beta_0 + \beta_1 \text{Radio}_{it} + \mu_i + \gamma_t + \epsilon_{it} \quad (4.6)$$

where  $Y_{it}$  corresponds to the retail margin of station  $i$  at time  $t$  and  $\text{Radio}_{it}$  is a dummy equal to one, if petrol station  $i$  lies in the reception area of a radio station broadcasting local petrol prices at date  $t$ .  $\mu_i$  are petrol station fixed effects, and  $\gamma_t$  are date fixed effects.

Since we only have information on radio reports in Bavaria, we restrict our analysis to petrol stations in Bavaria. We can thus exclude that petrol stations in the control group are affected by reports of radio stations we have not surveyed. We restrict our analysis to the period October 2013 until September 2014, which is the twelve months after the beginning of the test phase of the MTU.

To estimate the causal effects of radio reports on retail margins, we need to ensure that there are no spillovers of radio reports onto petrol stations in the control group and that the decision of radio stations to report was not because they anticipated evolutions in their local market that would also affect petrol prices. As we will see below, radio stations starting reports about petrol prices in Bavaria are located in urban areas. Although we control for differences in levels between the petrol stations using petrol station fixed effects, we are estimating an average treatment effect on the treated.

There are two possibilities which could lead to spillover effects between the treatment and control groups: Firstly, motorists outside of the reception area of the radio station could listen to the radio station via the internet. Secondly, commuters driving through the reception area of the radio station could update their information set by listening to the broadcasts and change their behavior accordingly after leaving the reception area. Both of these threats to identification are unlikely to be strong. Radio stations were still predominantly listened to via short-wave in 2013 and 2014. In particular, in more rural areas, mobile internet reception was still weak, making it difficult to listen to radio via the internet when on the road. Furthermore, although commuters learn something about the distribution of prices by listening to the radio, which may still be valuable outside the reception area, the value of this information is likely decreasing with distance to the reception area. In any event, both concerns lead to the control group being partially treated and would thus lead us to underestimate the treatment effect.

Another potential threat to identification could be that radio stations anticipated

a trend that would create local demand for reports about petrol prices and that also affected petrol prices. However, this seems unlikely. After multiple interviews, we learned from program directors that the decision of broadcasting petrol prices was not made based on a market analysis, but rather on the fit of such a segment to the existing program.

We now turn to the radio stations that define our treatment group. We consider radio reports about petrol prices by *Extra-Radio*, which broadcasts in and around Hof, a city in North-Eastern Bavaria, close to the Czech border, and *Radio Arabella*, which is a radio station broadcasting in and around Munich. Whereas *Extra-Radio* broadcasted the lowest petrol prices in its reception area daily between 2 February 2014 and 5 March 2017, *Radio Arabella* started reporting the lowest prices several times a day on 25 April 2014 and reports are still ongoing at the time of writing.

Similar to our discussion in Section 4.5, the presence of a country border is important. In particular, the reception area of *Extra-Radio* is very close to the border with the Czech Republic, the focal city Hof being less than 10 kilometers away from the border. Since Germany and the Czech Republic are both members of the Schengen Area, there are no border controls and shopping in the neighboring country is frequent. Due to lower taxes and levies, petrol prices are consistently 20 Eurocent lower in the Czech Republic. It therefore seems plausible that independent of price reports by radio stations or the MTU, price-sensitive shoppers always buy petrol in the Czech Republic, whereas only inelastic consumers buy from petrol stations treated by *Extra-Radio*. We would therefore expect that reports by *Extra-Radio* have little to no effect on retail margins. To test this hypothesis, we estimate the regression model for both radio stations separately. In each of these regressions we exclude petrol stations within the reception area of the other radio station from the control group.

Table 4.3 shows the results from the regression analysis. The regression in column (1) classifies petrol stations in the reception area of *Extra-Radio* and *Radio Arabella* as treated. Using this identification strategy, we find that radio reports on average decreased retail margins by 0.4 Eurocent. This effect is statistically significant at the 1 percent level. Columns (2) and (3) show the results of regressions only classifying petrol stations in the reception areas of *Extra-Radio* and *Radio Arabella* respectively as treated. For *Extra-Radio* we find a positive coefficient, but which is statistically indistinguishable from zero. For *Radio Arabella*, we find a statistically significant decrease in retail margins of 0.6 Eurocent. This suggests that, as expected, consumers and petrol stations in the reception area of *Extra-Radio* did not react to radio broadcasts of petrol prices, whereas consumers and petrol stations in the reception area of *Radio Arabella* did.

**Table 4.3:** The effect of radio reports on retail margins

Treatment group:	Both	Extra-Radio	Radio Arabella
Radio reports	-0.42*** (0.08)	0.56 (0.35)	-0.56*** (0.05)
Date fixed effects	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Station fixed effects	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Observations	516,416	455,801	502,269
Adjusted $R^2$	0.515	0.509	0.523
Mean retail margin	6.54	6.65	6.51

Notes: There are 71 petrol stations in the reception area of *Extra-Radio* and 279 petrol stations in the reception area of *Radio Arabella*. Column (1) compares retail margins of petrol stations in the reception areas of *Radio Arabella* and *Extra-Radio* to retail margins of other petrol stations in Bavaria before and after the beginning of petrol price reports. Column (2) compares the retail margins of petrol stations in the reception area of *Extra-Radio* to retail margins of other petrol stations in Bavaria before and after the beginning of petrol price reports. Column (3) compares the retail margins of petrol stations in the reception area of *Radio Arabella* to retail margins of other petrol stations in Bavaria before and after the beginning of petrol price reports.

Standard errors, clustered at the petrol station level, are in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

#### 4.6.2 The Role of Ex Ante Consumer Transparency

The second result of our theoretical framework is that the level of ex ante consumer transparency plays an important role in whether a marginal increase in transparency makes collusive behavior more or less profitable. In particular, we found that there is a U-shaped relationship between consumer transparency and the profitability of collusive behavior. At the same time, increasing producer transparency always makes collusive behavior more profitable.

Furthermore, we showed in Section 4.3 that the candidate theoretical models to explain the effect of mandatory price disclosure differ in their predictions of how the marginal effect of consumer information changes with the level of ex ante informed consumers. Providing empirical evidence on the effect of the MTU conditional on the share of ex ante informed consumers may thus help assess how well the different models fit a particular market.

An important empirical challenge is how to measure the share of informed consumers among potential customers of a petrol station. One possibility is to estimate the spatial distribution of the intensity of the use of smartphone applications, as does for example Luco (2019). There are three reasons why this is not appropriate in our setting: Firstly, we are interested in the share of ex ante informed consumers and thus



want a measure of consumer information before mandatory price disclosure was introduced. Secondly, the use of price comparison applications is likely endogenous, as it could be driven by the price level or the amount of price dispersion in a market. Finally, since there are many popular price comparison applications in Germany, getting a full picture of the spatial distribution of search would require to get search data from many different smartphone applications.

We therefore construct a measure of ex ante consumer information that is determined before the introduction of the MTU and that is exogenous to local petrol prices. We do this by using the share of commuters among potential customers of a petrol station on 30 June 2013 as the share of ex ante informed consumers. In this respect, we follow Pennerstorfer et al. (2019), who study the effect of the share of commuters on petrol prices in Austria. The underlying assumption is that out-of-municipality commuters are perfectly informed about prices of petrol stations along their daily way to work, whereas motorists that live in a municipality, but do not commute outside of their municipality, are uninformed potential customers of petrol stations in their municipality.<sup>43</sup>

In an ideal scenario, we would re-estimate the regression model in Section 4.5 and introduce interactions between the MTU introduction and the share of commuters. Unfortunately, commuter data for France is not available and we must thus resort to an alternative identification strategy. To this end, we exploit the daily price cycles and consumption patterns to identify times of the day which are treated by the introduction of mandatory price disclosure and others that are not.

Price cycles with low demand and high prices in the morning and high demand and low prices in the late afternoon suggest that petrol stations are practicing third-degree price discrimination using intertemporal differences in price.<sup>44</sup> In general, the demand for petrol is low and inelastic in the morning, because most motorists are on their way to work and have little time to spare to shop for petrol. They therefore only buy petrol if it is absolutely necessary. In contrast, in the late afternoon, motorists are on their way back from work, have more time to fuel and shop for low prices and demand is thus higher and more elastic. A profit maximizing oligopolist that can charge different groups different prices would charge the low elasticity group a higher price.<sup>45</sup> In our case, the different groups are consumers buying petrol at different times of the

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<sup>43</sup> We follow Pennerstorfer et al. (2019) and exclude petrol stations in municipalities with more than 1 million inhabitants in 2013 (Berlin, Hamburg, Munich and Cologne). In Germany, like in Austria, cities are treated as a single municipality. Using out-of-municipality commuters as a proxy for informed consumers is therefore more precise outside of very large cities.

<sup>44</sup> Figures D.5 and D.6 in Appendix D.3 describe daily fueling patterns and price patterns.

<sup>45</sup> Holmes (1989) extends the literature on third-degree price discrimination from the monopoly to the oligopoly case and shows that oligopolists would also want to engage in such behavior.

day. We should thus observe higher prices in the morning than in the late afternoon if consumers in the morning react less elastically to price changes. This is in line with the data and validates the assumption that demand in the morning is less elastic than in the afternoon. From this, we derive our identifying assumption that retail margins at 9 am are not affected by the introduction of the MTU, whereas retail margins at 5 pm are, and thus can serve as a control group.

We hence estimate the effect that mandatory price disclosure has on petrol prices at 5 pm using this alternative identification strategy. Doing so also allows us to check whether the estimated effect is in line with the results in Section 4.5. Specifically, we estimate the following fixed effects regression:

$$Y_{ith} = \beta_0 + \beta_1 MTU_{th} + \mu_{ih} + \gamma_t + \epsilon_{ith} \quad (4.7)$$

where  $Y_{ith}$  corresponds to the retail margin of station  $i$  at date  $t$  at hour  $h$  and  $MTU_{th}$  is a dummy equal to one, if petrol stations have to report prices to the MTU at date  $t$  and the price under consideration is reported at 5 pm.  $\mu_{ih}$  are petrol station-hour fixed effects, and  $\gamma_t$  are date fixed effects.

To estimate heterogeneities in the treatment effect driven by differences in the ex ante share of informed consumers, we interact the treatment effect with the share of commuters among potential customers of a station. We hence estimate the following model:

$$Y_{ith} = \alpha_0 + \alpha_1 MTU_{th} + \alpha_2 MTU_{th} \times comm_i + \alpha_3 MTU_{th} \times comm_i^2 + \mu_{ih} + \gamma_t + \epsilon_{ith} \quad (4.8)$$

where  $comm_i$  is the share of commuters among potential customers of a petrol station and all other variables are specified as in Equation 4.7.

Table 4.4 contains the results of the different estimations. In column (1), we find that the effect of mandatory price disclosure on retail margins is 1.8 Eurocent if we split treatment and control group according to the time of day. The estimate may be slightly upward biased, because if some elastic consumers switched from buying fuel in the morning to buying fuel in the evening after having the additional information from the MTU, this would further decrease the absolute value of the average price elasticity of demand in the morning and thus lead to a higher optimal price in equilibrium. In any event, the estimated effect of the introduction of mandatory price disclosure in column (1) is in line with the findings in Section 4.5.

In column (2), the treatment effect is interacted with the first- and second-order polynomial of the share of commuters among potential customers of a petrol station. The results suggest that in the case of the MTU, mandatory price disclosure is always

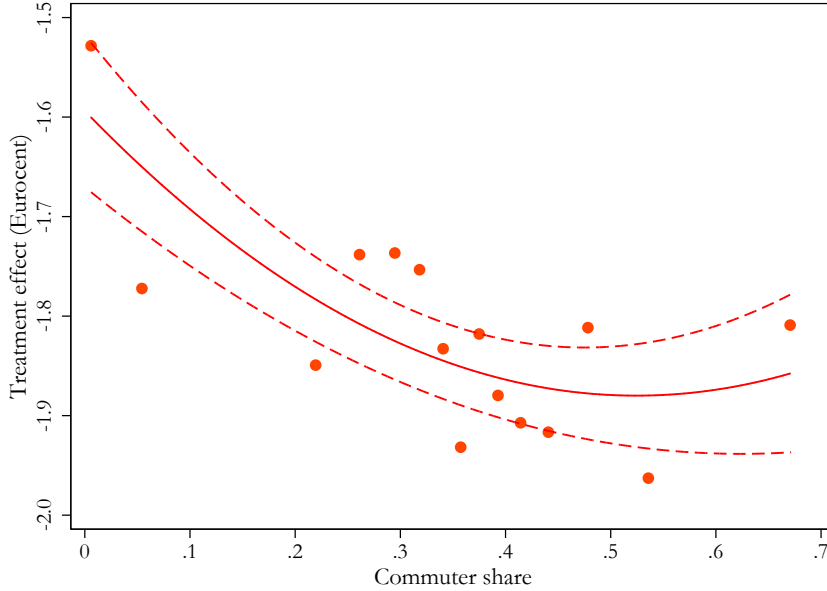
**Table 4.4:** The effect of the MTU on retail margins by commuter share

	(1)	(2)
MTU introduction	-1.82*** (0.02)	-1.59*** (0.04)
MTU $\times$ commuter share		-1.09*** (0.21)
MTU $\times$ commuter share <sup>2</sup>		1.04*** (0.31)
Date fixed effects	Yes	Yes
Station-hour fixed effects	Yes	Yes
Observations	7,448,175	7,448,175
Adjusted $R^2$	0.642	0.643
Mean retail margin	8.67	8.67

Notes: The test phase of the MTU (12 September 2013 until 30 November 2013) is dropped in all specifications. Petrol stations in municipalities with more than 1 million inhabitants in 2013 (Berlin, Hamburg, Munich and Cologne) are dropped in all specifications. Column (1) considers margins at 5 pm as the treatment and margins at 9 am as the control group, using data until 30 September 2014. Column (2) interacts the treatment dummy with the share of commuters among potential customers, using data until 30 September 2014. The commuter share is measured between 0 and 1.

Standard errors, clustered at the petrol station level, are in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Figure 4.3:** Treatment effect of the MTU by share of commuters

Notes: We exclude petrol stations in municipalities with more than 1 million inhabitants in 2013 (Berlin, Hamburg, Munich and Cologne) and split petrol stations into 15 quantiles of equal size according to the commuter share. Each point shows the average effect of mandatory price disclosure for a particular quantile. The solid line plots the estimated treatment effect of the MTU depending on the percentage share of commuters of a petrol station based on the regression coefficients in column (2) of Table 4.4. The dashed lines represent the 95% confidence interval.

pro-competitive, but more so for higher levels of ex ante informed consumers.

To allow for a more flexible relationship, we split petrol stations into 15 quantiles of equal size according to the share of commuters among potential customers. We then estimate the treatment effect of the MTU separately for each quantile. Figure 4.3 plots the treatment effects for the different commuter share quantiles. Furthermore, the solid line plots the estimated effect of the MTU by commuter share from column (2) in Table 4.4. The results indicate that there is a downward-sloping relationship between the commuter share and the treatment effect.

Relating these results to predictions of different theoretical models allows us to assess the suitability of these theoretical models in our setting. Unless we assume that marginal consumers become more easy to inform the more consumers are already informed, we would expect to see an upward-sloping relationship between the commuter share and the treatment effect in the absence of collusive behavior. The results therefore suggest that incorporating collusive behavior into the theoretical model is necessary.

Next, we can relate the empirical results to the predictions about the effect of mandatory price disclosure in the different theoretical models that include collusive

behavior. If we assume that the effect of mandatory price disclosure on producer transparency is orthogonal to the commuter share, then we can interpret the heterogeneity in the treatment effect as heterogeneity of the effect of increasing consumer transparency depending on different levels of ex ante consumer information. In Section 4.3, we discuss how the different theoretical models give rise to different predictions for the effect of increasing consumer transparency depending on the ex ante level of consumer transparency.

Unless we assume that the share of new consumers becoming informed after mandatory price disclosure increases in the share of ex ante consumer transparency, the empirical results suggest that for most petrol stations, the absolute value of the marginal effect of increasing consumer transparency increases with the share of ex ante consumer transparency. This is in line with the predictions of the theoretical model in Section 4.3 for intermediate levels of ex ante consumer transparency. It is at odds with the predictions in the theoretical model for homogeneous goods of Schultz (2017) or for differentiated goods of Schultz (2017) and Luco (2019). At the same time, the empirical results suggest that for high levels of ex ante consumer transparency the magnitude of the treatment effect decreases again. This is also consistent with the predictions for high levels of ex ante consumer transparency in the theoretical model in Section 4.3.

## 4.7 Discussion

When evaluating the effect of mandatory price disclosure, it is crucial to understand how it affects consumer and producer information, as well as how a marginal increase of producer and consumer information affects prices. The results in this paper suggest that the levels of ex ante consumer and producer information are important to assess the effect of a marginal increase in information on prices. The theoretical model in Section 4.3 shows how changes in consumer, producer and common information affect the stability of collusive behavior. It can thus guide policymakers in assessing the effect of mandatory price disclosure under different market conditions.

To be confident in the predictions of the theoretical model, we need to assess how well it fits the particular market. We can do this in two ways: Firstly, by assessing whether the assumptions of the model fit the qualitative description of the market. Secondly, by evaluating whether the theoretical predictions of the model fit the empirical evidence. We follow both of these approaches.

In Section 4.2, we describe the German retail petrol market and assess the assumptions of the model qualitatively. This suggests that retail petrol is a homogeneous good and that there are informational frictions on both sides of the market. Although some consumers are imperfectly informed, they still have the possibility to search for

price information by driving to different stations. Furthermore, there is evidence collected by the GFCO suggesting that firms engage in collusive behavior. Like in the theoretical models by Schultz (2017) and Luco (2019), we therefore consider a model where both sides of the market are imperfectly informed and collusive behavior is possible. In contrast to these models, we allow uninformed consumers to search sequentially.<sup>46</sup> In contrast to Luco (2019), we model retail petrol as a homogeneous good.

Despite the qualitative evidence of collusive behavior, one might nevertheless be worried that focusing on models with stable collusion above the critical discount factor is not suitable because we observe frequent price changes in the data. However, although collusion on the critical discount factor does not capture all the dynamics present in the market, there is abundant evidence suggesting that collusive behavior plays an important role in the retail petrol market. Firstly, as outlined in Section 4.2, the market investigation by the GFCO found substantial evidence consistent with collusive behavior. Secondly, our results regarding the effect of mandatory price disclosure and the ex ante share of informed consumers are in line with collusive behavior rather than competition. Thirdly, as shown by Byrne and de Roos, 2019, tacit collusion in the retail petrol market often involves complex pricing strategies and learning to coordinate on a focal price. Observing frequent price changes is hence not evidence of the absence of collusive behavior.

Alternatively, one could look at different models of collusive behavior which generate price cycles, as in Maskin and Tirole (1988), or price wars, as in Green and Porter (1984). Although these models allow for richer collusive strategies, they are more restrictive in the modeling of information and search. Since information and search are crucial when studying mandatory price disclosure, we thus choose to be more flexible in modeling these aspects. Like in Cabral et al. (2019), comparative statics with respect to the critical discount factor should thus be seen as changes in the likelihood of collusive behavior. Although the model cannot exactly predict how equilibrium pricing changes after the introduction of mandatory price disclosure, it allows inferring how mandatory price disclosure changes the likelihood of being in a high price phase.

In Section 4.6, we assess whether the empirical evidence is consistent with the predictions of the theoretical model. We find that the size of the marginal treatment effect increases with the level of ex ante consumer transparency for low and medium levels of ex ante consumer transparency, and decreases for high levels. This is consistent with the predictions of the theoretical model in Section 4.3, but does not match the

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<sup>46</sup> Petrikaite (2016) also allows for uninformed consumers to search sequentially, but assumes that firms are perfectly informed.

predictions in the models by Schultz (2017) and Luco (2019). One might be worried about not observing price increases for low commuter shares, since the theoretical model predicts that for low levels of consumer transparency a marginal increase in consumer transparency stabilizes collusion. There are two potential explanations for this: Firstly, the commuter share is an imperfect proxy for consumer transparency and thus even if the share of commuters is zero, there are likely to be some informed consumers. Secondly, the change in consumer information caused by mandatory price disclosure is unlikely to be marginal. Thus, even if a marginal increase in consumer information stabilizes collusion, the increase in consumer information caused by mandatory price disclosure can be pro-competitive. In this case, our empirical results remain consistent with the predictions of the theoretical model in Section 4.3, and at odds with the predictions of other candidate models.

Although the model in Section 4.3 seems a better fit for the German retail petrol market than other candidate models, this does not have to be the case for other markets. The predictions in Section 4.3 can guide future researchers in distinguishing whether this model or models for differentiated products, such as in Schultz (2017) or Luco (2019), are more suitable. Other candidate models for homogeneous goods are nested within the model in this paper. By setting  $\eta = 1$ , the model corresponds to the homogeneous goods model by Petrikaite (2016). If  $s = 0$ , the model corresponds to the homogeneous goods model by Schultz (2017). By setting  $\eta = 1$  and  $s = 0$ , the model corresponds to the homogeneous goods model by Schultz (2005).

Empirically, we find robust evidence that mandatory price disclosure benefited consumers and led to a decrease in retail margins by 13 percent. A key feature of studying the introduction of the MTU is that price information is salient, the homogeneity of the good allows for an easy comparison of prices and the price information was adopted by many consumers. As shown in Figure D.7, by December 2014, three mobile price comparison applications for which we have usage data already had more than 70 million monthly page impressions.<sup>47</sup> We thus estimate the effect of mandatory price disclosure conditional on the disclosed information being adopted by consumers.

A potential concern could be that the drop in the price of crude oil in the second half of 2014 could bias our results. Since we analyze retail margins, which are net of input prices, and control for station and date fixed effects, this would require the pass-through of input prices to change differently for the treatment and the control group over time. This is unlikely to be a concern because most of our analysis only uses data until 30 September 2014, whereas the largest share of the decrease in the

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<sup>47</sup> This is a lower-bound and likely to be a strong understatement of the actual usage of price comparison applications, since there were approximately 120 information service providers registered with the GFCO by the end of 2014.

price of crude oil occurred between October and December 2014. Furthermore, our data set allows us to robustly estimate the treatment effect using different treatment groups and different identification strategies. In particular, the effect found when only using data for Germany and exploiting third-degree price discrimination according to the time of day to identify treatment and control groups cannot be explained by this.

The high adoption rate of information from the MTU by consumers also sets this analysis apart from the setting in other studies. This is likely to be an important driver of the difference in results found in contrast to the empirical results by Luco (2019). He shows that mandatory price disclosure in the Chilean petrol industry led to an increase in retail margins by 9 percent. However, he also finds that only few consumers in Chile accessed this information. The vastly different empirical results highlight the importance of obtaining a distribution of estimates of the effect of mandatory price disclosure in different settings. The average effect found in one setting often does not inform about the effect in another setting. Understanding the underlying mechanisms that drive the effect is thus indispensable.

Our data allow us to go further than the existing literature in studying these different mechanisms empirically. In particular, the information about local radio reports allows us to study the effect of media reports about petrol prices after mandatory price disclosure is introduced. The administrative data on commuter streams allows us to construct a proxy for the ex ante share of informed consumers which is determined before the introduction of mandatory price disclosure and exogenous to prices.

There are three main findings, which are relevant for policymakers. Firstly, the model unambiguously predicts that, if nothing else changes, increasing price transparency among producers is anti-competitive. Since we do not observe differences in producer transparency in the German retail petrol market before the introduction of mandatory price disclosure, we cannot study this mechanism empirically. Contrasting the setting and results in our study to the literature can provide some helpful insights. Whereas Luco (2019) notes that mandatory price disclosure likely improved the information set of producers significantly, there is a lot of evidence suggesting that producers were already well informed before the introduction of the MTU in Germany and thus its introduction had little effect on producer transparency.<sup>48</sup>

Secondly, another important result of the theoretical model is that even if firms are perfectly informed ex ante, an increase in price transparency can stabilize collusive behavior. In particular, we find that the marginal effect of increasing consumer transparency depends on the share of consumers that are perfectly informed about prices ex ante. The empirical evidence supports these predictions.

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<sup>48</sup> This is discussed in depth by the GFCO (2011).



Finally, the model predicts that, holding everything else equal, if a small increase in consumer price transparency is pro-competitive, then a large increase will be even more so. We provide empirical evidence on the effect of increasing the share of informed consumers whilst holding everything else constant, by studying the effect of radio stations reporting petrol prices after the introduction of the MTU. Our results suggest that media reports and information campaigns can play a crucial role in raising the share of informed consumers. This is in line with results by Ater and Rigbi (2018), who find that whilst consumers only made little use of price comparison sites for supermarkets after mandatory price disclosure was introduced in Israel, a key driver for the pro-competitive effects of this policy were media reports ranking chains according to the results of price comparisons.

#### 4.8 Conclusion

As we have shown for the case of the MTU in Germany, introducing mandatory price disclosure in homogeneous goods markets can be beneficial to consumers. By disentangling the mechanisms, both theoretically and empirically, we have also shown, that this is not a one-size-fits-all solution to remedy competition concerns in markets with informational frictions. Under certain conditions mandatory price disclosure has a pro-competitive effect, whereas under other conditions its effect is anti-competitive.

Based on our analysis, we offer two recommendations for policymakers: Firstly, policymakers should assess the level of information of producers before introducing mandatory price disclosure. If producer information is low *ex ante*, then mandatory price disclosure could potentially lead to higher prices. If this is already very high *ex ante*, then mandatory price disclosure is unlikely to harm consumers further through this channel. It may, however, still do so through changes in consumer transparency. Secondly, if policymakers decide to mandate price disclosure, they should not only make price information available, but also push for large-scale adoption by consumers to fully reap the pro-competitive effects. This could, for example, be done through public information campaigns or media reports.

## Appendix D

### D.1 Static Equilibrium

Since all firms are symmetric, we solve the static game by looking for the symmetric Nash-equilibrium in mixed strategies. As we will see, the solution fulfilling the equilibrium conditions is unique.

Using an optimal stopping rule, the reservation price is such that the marginal gain of search for non-shoppers equals their marginal cost and must therefore solve:

$$\int_p^{p_r} (p_r - p) dG(p) = s. \quad (4.9)$$

It is increasing in  $s$ , because the higher the incremental search cost, the higher is the price above which it is not profitable for non-shoppers to engage in sequential search. For the remainder of the analysis, we focus on the case where the search cost  $s$  is such that the reservation price does not exceed the valuation, i.e.  $p_r \leq v$ . When this condition does not hold, it is not the reservation price that binds firm pricing but the valuation  $v$ .

Non-shoppers are homogeneous in their valuation of the good and search costs and therefore have the same reservation price. Since sellers never choose a price above the reservation price, non-shoppers never search in equilibrium and always go to the shop they know the price of already.

To derive the equilibrium price distribution  $G(p)$ , we take advantage of the equi-profit condition, by which in a Nash equilibrium, all prices that are played with positive probability must yield the same expected profit. Shoppers will always buy at the cheapest firm, whereas non-shoppers buy at the firm they randomly draw a price from. The probability that a firm setting price  $p_i$  attracts shoppers is  $(1 - G(p_i))^{n-1}$ , which is the probability that all other firms set a higher price than firm  $i$ .<sup>49</sup> The expected profit of firm  $i$  is therefore:

$$\pi_i = \left( \phi(1 - G(p_i))^{n-1} + \frac{1 - \phi}{n} \right) p_i. \quad (4.10)$$

The first part of the equation is the sum of the share of shoppers in the population multiplied by the probability that firm  $i$  attracts these shoppers and the share of non-shoppers in the population divided by the number of firms (i.e. the share of non-shoppers shopping at firm  $i$ ). This is multiplied by the price chosen by firm  $i$  and the expected market size of one. For firms selling at the reservation price  $p_r$  this boils down

<sup>49</sup> Note, that since prices are continuously distributed and the distribution has no mass point, the probability of two firms setting exactly the same price and sharing shoppers is zero.

to  $\pi(p_r) = p_r \frac{1-\phi}{n}$ , because the probability that no other firm chooses a lower price is zero and thus  $G(p_r) = 1$ .

Since by the equiprofit condition the expected profits of firm  $i$  have to be constant for all prices it chooses with positive probability, in equilibrium  $\pi(p) = \pi(p_r)$ . Solving this equation for the distribution of prices yields:

$$G(p) = 1 - \left( \frac{(p_r - p)(1 - \phi)}{np\phi} \right)^{\frac{1}{n-1}}. \quad (4.11)$$

Finally, we can also use the equiprofit condition to derive the lower-bound price  $\underline{p}$ . Expected profits of charging  $p_r$  or  $\underline{p}$  must, again, be equal. This time we can exploit the fact that the likelihood that any firm chooses a price below the lower-bound price is zero and thus the likelihood that a firm charging  $\underline{p}$  attracts all shoppers is  $G(\underline{p}) = 1$ . Setting  $\pi(\underline{p}) = \pi(p_r)$  and solving for  $\underline{p}$  yields:

$$\underline{p} = \frac{p_r(1 - \phi)}{1 + (n - 1)\phi}. \quad (4.12)$$

The competitive profit  $\pi^*$  in the dynamic model is therefore the expression in Equation 4.10, where  $p_i$  is drawn from a distribution  $G(p)$  over the domain  $[\underline{p}, p_r]$ .  $G(p)$ ,  $\underline{p}$  and  $p_r$  are the unique solution to Equations 4.9, 4.11 and 4.12. Together, the optimal stopping rule of non-shoppers and the equiprofit conditions are thus sufficient to characterize the unique equilibrium of the one-shot Nash equilibrium game.

## D.2 Proof of Propositions

*Proof of Proposition 1.* Producer transparency is modeled as the likelihood that a deviation from the collusionary agreement is detected by rivals.  $\eta$  does not enter the profit function and only increases the likelihood of entering a punishment phase in the next period if the firm deviates. It hence follows that increasing transparency on the producer side always fosters collusive behavior:

$$\frac{\partial \delta}{\partial \eta} = - \frac{\pi^c - \pi^*}{(\pi^d - \eta\pi^* - (1 - \eta)\pi^c)^2} < 0. \quad (4.13)$$

□

*Proof of Proposition 2.* In a first step, we differentiate the critical discount factor with

respect to  $\phi$  and get:

$$\frac{d\delta}{d\phi} = \frac{(\frac{\partial\pi^d}{\partial\phi} - \frac{\partial\pi^c}{\partial\phi})(\pi^d - \eta\pi^* - (1-\eta)\pi^c) - (\frac{\partial\pi^d}{\partial\phi} - \eta\frac{\partial\pi^*}{\partial\phi} - (1-\eta)\frac{\partial\pi^c}{\partial\phi})(\pi^d - \pi^c)}{(\pi^d - \eta\pi^* - (1-\eta)\pi^c)^2}. \quad (4.14)$$

To prove Proposition 2, we need to determine the sign of Equation 4.14. Since the denominator is always positive, we can focus our attention on the numerator. After some simplifications the numerator becomes:

$$\left[ \frac{\partial\pi^d}{\partial\phi}(\pi^c - \pi^*) + \frac{\partial\pi^*}{\partial\phi}(\pi^d - \pi^c) \right] \eta. \quad (4.15)$$

We focus on the case where  $\eta > 0$ , i.e. a deviation from the collusive price is observed by rivals with a strictly positive probability. If this was not the case, then increasing the share of shoppers  $\phi$  would have no effect on the critical discount factor. With  $\eta > 0$ , we do not need to consider  $\eta$  further to determine the sign of Equation 4.15. Instead, we analyze the rest of the expression, which becomes:

$$\begin{aligned} & \frac{\partial\pi^d}{\partial\phi}(\pi^c - \pi^*) + \frac{\partial\pi^*}{\partial\phi}(\pi^d - \pi^c) = \\ & v\left(1 - \frac{1}{n}\right)\left(\frac{v}{n} - \frac{p_r(1-\phi)}{n}\right) + v\left(\frac{\partial p_r}{\partial\phi} \frac{1-\phi}{n} - \frac{p_r}{n}\right)\left(\phi - \frac{\phi}{n}\right) = \\ & \frac{v}{n^2}(n-1)\left[v - p_r + \frac{\partial p_r}{\partial\phi}(1-\phi)\phi\right]. \end{aligned} \quad (4.16)$$

To determine the sign of this expression, we need an expression for the reservation price and its derivative with respect to the share of shoppers  $\phi$ .<sup>50</sup> In equilibrium, Equation 4.9 becomes:

$$\begin{aligned} & \frac{1}{n-1}\left(\frac{1-\phi}{n\phi}\right)^{\frac{1}{n-1}} \int_{\frac{p_r(1-\phi)}{1+(n-1)\phi}}^{p_r} \left(\frac{p_r}{p} - 1\right)^{\frac{1}{n-1}} \frac{p_r}{p} dp = \\ & \frac{1}{n-1}\left(\frac{1-\phi}{n\phi}\right)^{\frac{1}{n-1}} p_r \int_{\frac{1(1-\phi)}{1+(n-1)\phi}}^1 \left(\frac{1}{t} - 1\right)^{\frac{1}{n-1}} \frac{1}{t} dt. \end{aligned} \quad (4.17)$$

If  $n = 2$ , then Equation 4.17 simplifies to:

$$\frac{1-\phi}{2\phi} p_r \int_{\frac{1-\phi}{1+\phi}}^1 \left(\frac{1}{t} - 1\right) \frac{1}{t} dt = p_r \left(\frac{1-\phi}{2\phi} \ln\left(\frac{1-\phi}{1+\phi}\right) + 1\right) = s. \quad (4.18)$$

<sup>50</sup> The remainder of the proof closely follows Petrikaite (2016).

The reservation price therefore becomes:

$$p_r = s \left( \frac{1-\phi}{2\phi} \ln \left( \frac{1-\phi}{1+\phi} \right) + 1 \right)^{-1}, \quad (4.19)$$

and the derivative of the reservation price with respect to the share of shoppers is:

$$\frac{\partial p_r}{\partial \phi} = s \left( \frac{1-\phi}{2\phi} \ln \left( \frac{1-\phi}{1+\phi} \right) + 1 \right)^{-2} \times \left( \frac{2\phi + (\phi+1) \ln \left( \frac{1-\phi}{1+\phi} \right)}{2\phi^2(\phi+1)} \right). \quad (4.20)$$

We can also show that the reservation price decreases in the share of shoppers. The search cost  $s$ , the first bracket and the denominator of the second bracket are always positive. Furthermore, we can show that the derivative of  $2\phi + (\phi+1) \ln \left( \frac{1-\phi}{1+\phi} \right)$  with respect to  $\phi$  is negative:

$$-\frac{2\phi - (\phi-1) \ln \left( \frac{1-\phi}{1+\phi} \right)}{1-\phi} < 0. \quad (4.21)$$

Therefore, since  $\phi$  is defined on the domain  $[0, 1]$

$$2\phi + (\phi+1) \ln \left( \frac{1-\phi}{1+\phi} \right) \leq \lim_{\phi \rightarrow 0} \left( 2\phi + (\phi+1) \ln \left( \frac{1-\phi}{1+\phi} \right) \right) = 0, \quad (4.22)$$

and the derivative  $\frac{\partial p_r}{\partial \phi}$  is always negative.

Next, we plug the expression for  $\frac{\partial p_r}{\partial \phi}$  and  $s$  into Equation 4.16, set  $n = 2$  and multiply everything by  $\frac{4}{v}$ . As a result we obtain the following expression:

$$\begin{aligned} v - p_r + \left( \frac{\frac{2\phi + (\phi+1) \ln \left( \frac{1-\phi}{1+\phi} \right)}{2\phi^2(\phi+1)}}{\frac{1-\phi}{2\phi} \ln \left( \frac{1-\phi}{1+\phi} \right) + 1} \right) (1-\phi) p_r = \\ v - p_r \frac{4\phi^2}{(1+\phi)((1-\phi) \ln \left( \frac{1-\phi}{1+\phi} \right) + 2\phi)}. \end{aligned} \quad (4.23)$$

In line with Proposition 2, we now show how the sign of this expression depends on  $\phi$ . As shown above,  $\frac{\partial p_r}{\partial \phi}$  is always negative. Furthermore

$$\frac{\partial}{\partial \phi} \left( \frac{4\phi^2}{(1+\phi)((1-\phi) \ln \left( \frac{1-\phi}{1+\phi} \right) + 2\phi)} \right) = \frac{8\phi(2\phi + \ln \left( \frac{1-\phi}{1+\phi} \right))}{(1+\phi)^2((1-\phi) \ln \left( \frac{1-\phi}{1+\phi} \right) + 2\phi)^2} < 0, \quad (4.24)$$

where the inequality is obtained from the fact that

$$\frac{\partial}{\partial \phi} \left( 2\phi + \ln \left( \frac{1-\phi}{1+\phi} \right) \right) = 2 - \frac{2}{1-\phi^2} < 0 \quad (4.25)$$

and thus since  $\phi$  is defined on the domain  $[0, 1]$

$$2\phi + \ln \left( \frac{1-\phi}{1+\phi} \right) \leq \lim_{\phi \rightarrow 0} \left( 2\phi + \ln \left( \frac{1-\phi}{1+\phi} \right) \right) = 0. \quad (4.26)$$

We can therefore conclude that Equation 4.24 increases in  $\phi$ . By replacing  $p_r$  by its value, we can analyze how the second part of Equation 4.23 behaves in the limits of  $\phi$ :

$$\lim_{\phi \rightarrow 0} \left( p_r \frac{4\phi^2}{(1+\phi)((1-\phi)\ln(\frac{1-\phi}{1+\phi}) + 2\phi)} \right) = \infty \quad (4.27)$$

$$\lim_{\phi \rightarrow 1} \left( p_r \frac{4\phi^2}{(1+\phi)((1-\phi)\ln(\frac{1-\phi}{1+\phi}) + 2\phi)} \right) = s. \quad (4.28)$$

Since we focus on cases where the search cost  $s \leq p_r$ , this means that

$$s \leq \int_{p(v)}^v (v-p) dG(p). \quad (4.29)$$

Analogous to the steps to obtain Equation 4.19, for  $n = 2$  this inequality simplifies to

$$s \leq \left( \frac{1-\phi}{2\phi} \ln \left( \frac{1-\phi}{\phi+1} \right) + 1 \right) < v. \quad (4.30)$$

Since  $s < v$ , the derivative of the critical discount factor with respect to the share of shoppers  $\phi$  is negative for values of  $\phi \leq \bar{\phi}$  and positive for values of  $\phi > \bar{\phi}$ .

□

*Proof of Proposition 3.* The effect of a change in  $\alpha$  on the profitability of engaging in collusive behavior can be decomposed in the following way:

$$\frac{d\delta}{d\alpha} = \frac{\partial \delta}{\partial \eta} \frac{\partial \eta}{\partial \alpha} + \frac{\partial \delta}{\partial \phi} \frac{\partial \phi}{\partial \alpha}. \quad (4.31)$$

Since a change in  $\alpha$  increases the critical discount factor if, and only if,  $\frac{d\delta}{d\alpha} > 0$ , it follows that a marginal increase in  $\alpha$  is pro-competitive if, and only if:

$$\frac{e_{\eta, \alpha}}{e_{\phi, \alpha}} < - \frac{\frac{\partial \delta}{\partial \phi} \phi}{\frac{\partial \delta}{\partial \eta} \eta}. \quad (4.32)$$

$\phi$ ,  $\eta$ ,  $e_{\eta,\alpha}$  and  $e_{\phi,\alpha}$  are always positive and  $\frac{\partial \delta}{\partial \eta}$  is always negative.

If  $\frac{\partial \delta}{\partial \phi} < 0$ , then increasing  $\alpha$  is anti-competitive. We showed in the proof of Proposition 2 that this is the case for  $\phi \leq \bar{\phi}$ .

For an increase in common information  $\alpha$  to be pro-competitive, it is necessary that  $\frac{\partial \delta}{\partial \phi} > 0$ , which is the case for  $\phi \leq \bar{\phi}$ . Finally, the elasticity of  $\phi$  with respect to  $\alpha$  must be sufficiently large compared to the elasticity of  $\eta$  with respect to  $\alpha$ .

□

### D.3 Retail Margins and Petrol Station Characteristics in Germany

Figure D.1 shows the distribution of petrol stations in Germany over our sample period. Petrol stations are spread across the country and clustered around urban areas.

Table D.1 shows the share of the vertically integrated firms, as well as the share of non-integrated firms before and after the MTU introduction. Overall, the brand composition is very similar before and after the introduction of the MTU.

Figure D.2 shows the daily number of petrol stations for which the price panel contains a price entry at 5 pm. There is no structural break in the daily number of petrol stations for which there is an entry in the price panel before and after the MTU introduction. For most days in the pre-MTU period, we have prices for approximately 12,000 petrol stations in our panel. This number stays approximately the same after the introduction of the MTU and only increases to around 13,500 at the end of February 2014, when reporting issues of Total and Esso stop.<sup>51</sup> At any point in time over the observation period, our panel therefore includes prices for most of the approximately 14,700 petrol stations in Germany.

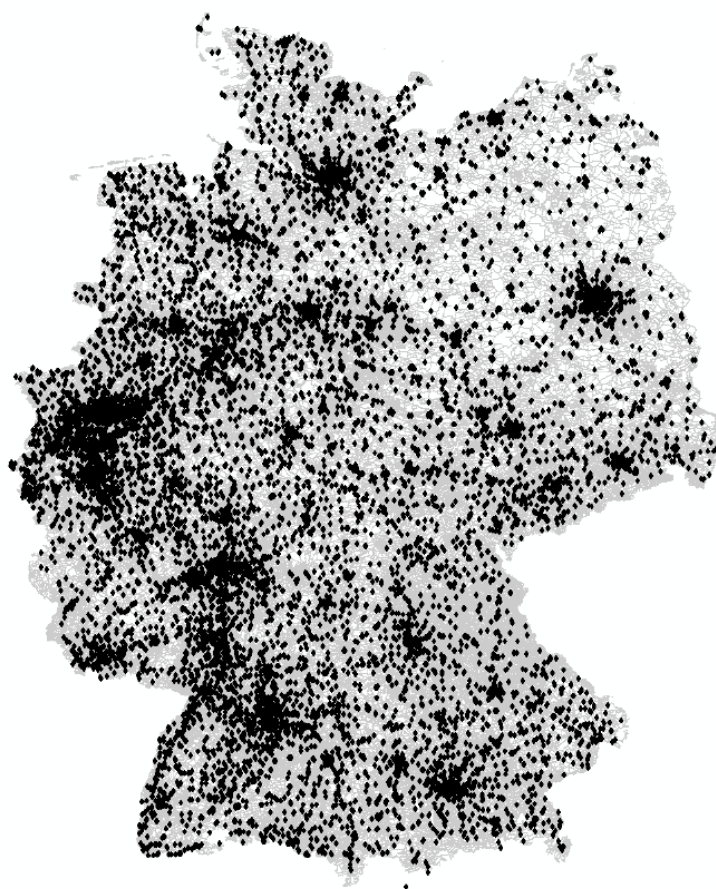
Figure D.3 shows that there are fewer price changes per day in our data prior to the MTU introduction than after the MTU was introduced. This is because whereas after the introduction of the MTU we observe the universe of price changes in Germany, before the introduction of the MTU we only observe the subset of prices that was reported by users to the app.

Figure D.4 shows the number of notifications of price changes over the day, before and after the introduction of the MTU. Whereas before the introduction of the MTU there is a notification every time a user of the app reports a price, after the MTU there is a notification every time that there is a price change.

Figure D.5 shows the hourly fueling patterns as reported in a representative survey among drivers commissioned by the German Federal Ministry of Economic

<sup>51</sup> Total and Esso report normally in October 2013. Esso reports only a very limited amount of prices between November 2013 and mid-February 2014. Total only reports a very limited amount of prices between December 2013 and mid-February 2014. Both experienced reporting issues in April 2014, after which they returned to full reporting.

**Figure D.1:** Distribution of petrol stations across Germany



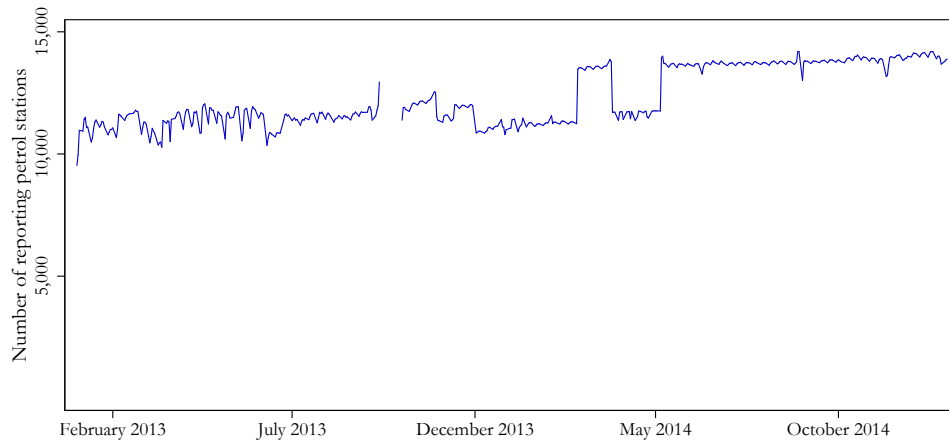
Note: The figure shows the geographic distribution of petrol stations in Germany.



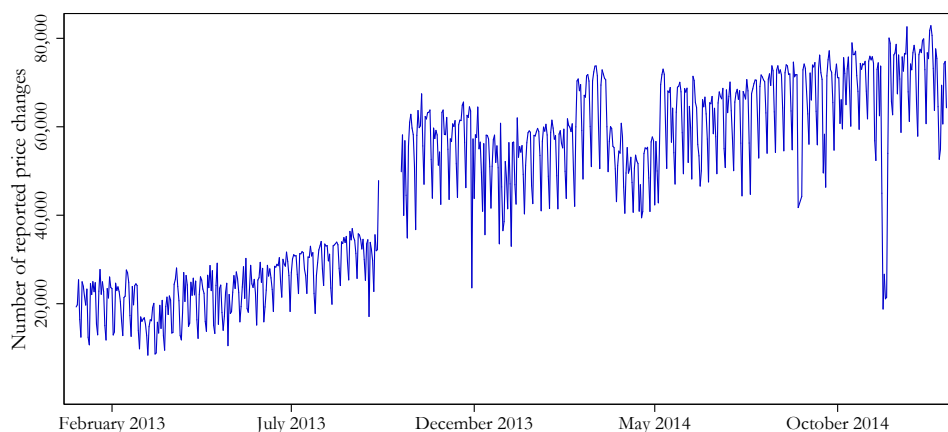
**Table D.1:** Share of stations in percent by brand

	Pre-MTU	Post-MTU
Aral	20.1	18.1
Shell	14.2	14.2
Esso	5.7	5.4
Total	7.0	4.7
Jet	5.0	4.6
Orlen	4.7	4.2
Agip	2.0	3.1
Hem	3.0	2.8
OMV	2.6	2.3
Non-integrated	35.8	40.6

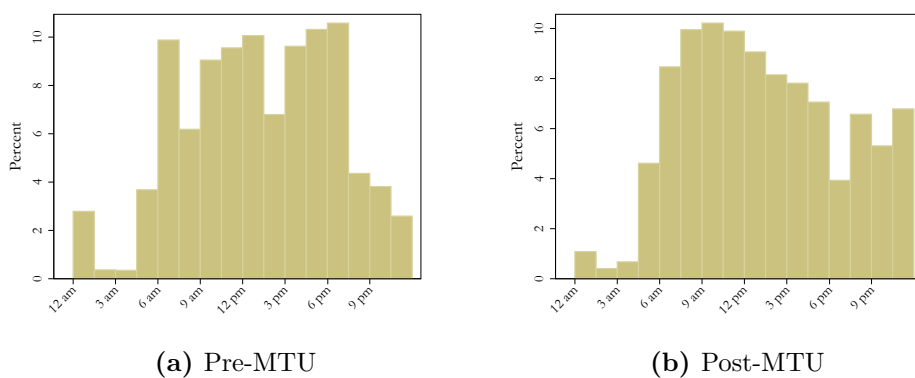
Notes: The “Pre-MTU” column shows the share of petrol stations by brand in the sample for Germany before the introduction of the MTU. The “Post-MTU” column shows the share of petrol stations by brand in the sample for Germany after the introduction of the MTU. We consider all petrol station that have at least one price entry in the sample before or after the MTU introduction, respectively.

**Figure D.2:** Number of petrol stations with positive price reports at 5pm

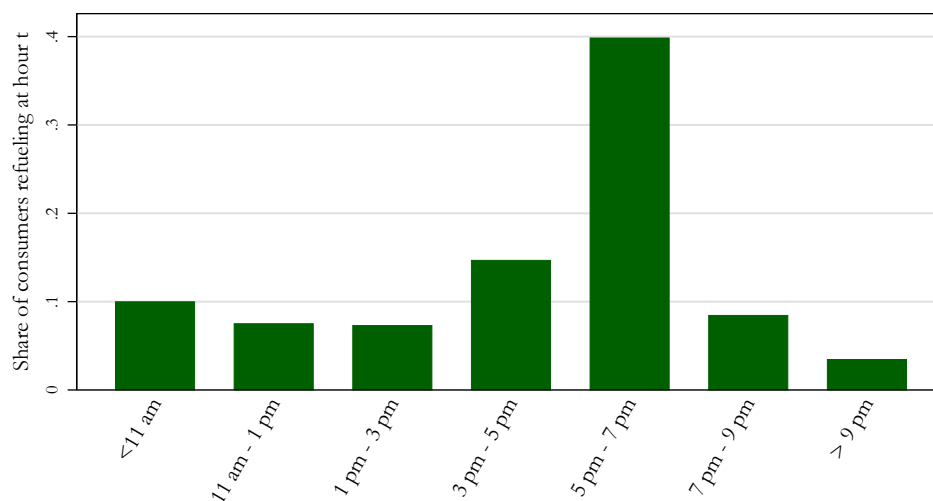
Notes: The Figure shows the average daily number of petrol stations with a positive price report at 5 pm in Germany in our sample.

**Figure D.3:** Number of daily price changes

Notes: The Figure shows the average daily number of price changes in Germany in our data. In the pre-MTU period consecutive reports of the same price are not considered a price change.

**Figure D.4:** Notification patterns over the day

Notes: Panel (a) shows the share of price notifications in our data set for every hour of the day for the pre-MTU period. Panel (b) shows the share of price notifications in our data set for every hour of the day for the post-MTU period. Pre-MTU, each price report by users notifying a price change to the information service provider is a price notification. Post-MTU, each price change notified by petrol stations to the MTU is a price notification.

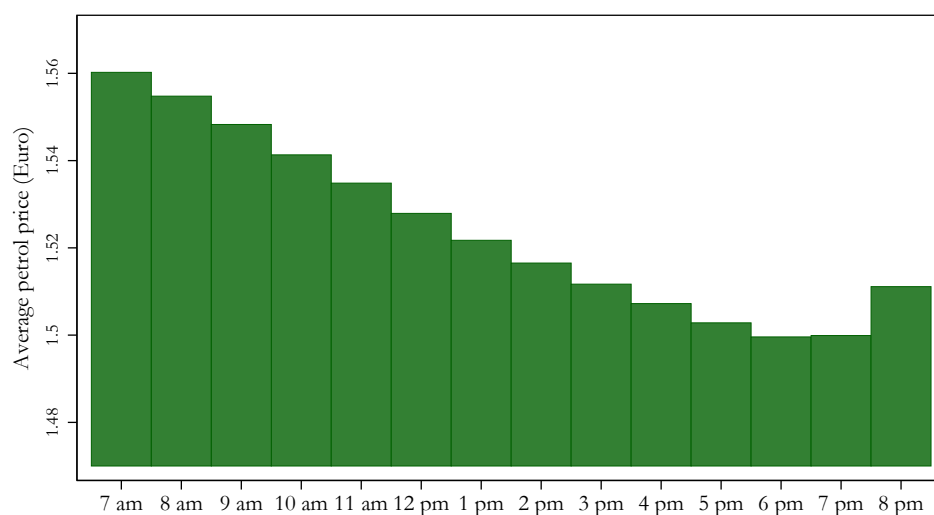
**Figure D.5:** Daily fuelling patterns

Notes: The figure shows the average fuelling patterns by German motorists over the day. Data is based on a representative survey among drivers commissioned by the German Federal Ministry of Economic Affairs.

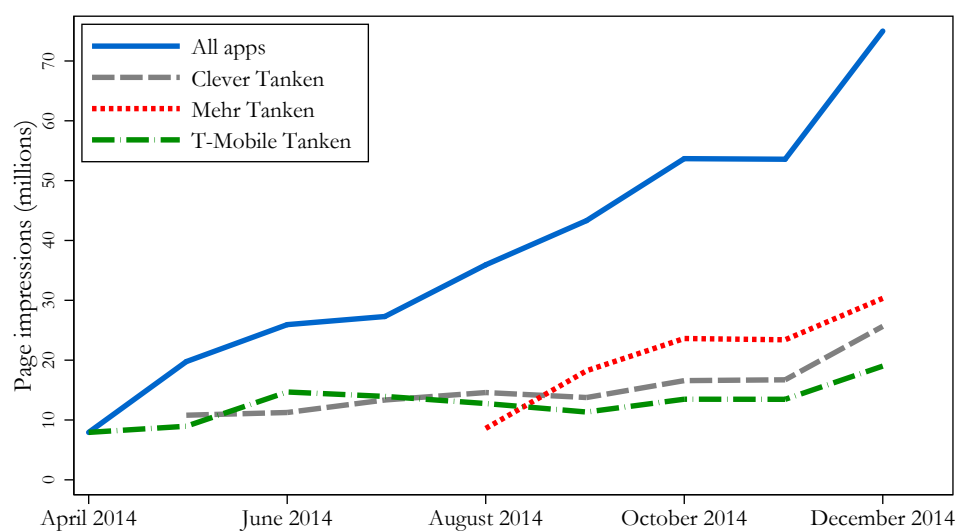
Affairs. As discussed in Section 4.6, the majority of drivers buy fuel between 5 pm and 7 pm, whereas only very few drivers buy petrol in the morning.

The fuelling patterns are also consistent with price patterns reported in Figure D.6. Whereas prices are highest in the morning, they fall during the day until the early evening and start rising again at around 8 pm.

Figure D.7 shows the evolution of monthly page impressions for three mobile price comparison applications for which data is available in 2014. Although these three mobile applications are only a fraction of the German mobile petrol price comparison market, they together have more than 70 million page impressions in December 2014. This shows that mobile price comparison applications were widely used by the end of 2014.

**Figure D.6:** Daily price patterns

Notes: The Figure shows the average petrol price for every hour between 7 am and 20 pm in Germany between 2013 and 2014.

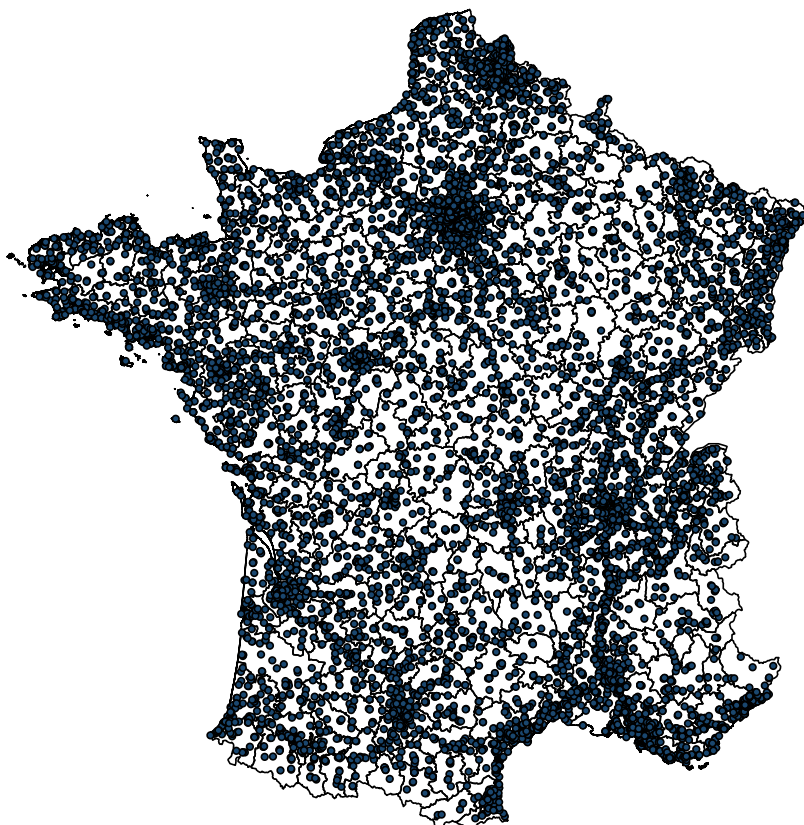
**Figure D.7:** Monthly page impressions

Notes: The Figure shows the evolution of monthly page impressions for three popular mobile price comparison applications. Each line begins when data for the particular app becomes available and ends at the end of our sample period, in December 2014.

#### D.4 Retail Margins and Petrol Station Characteristics in France

Figure D.8 shows the distribution of petrol stations in France over our sample period. As expected, petrol stations are spread across the country and clustered around urban areas.

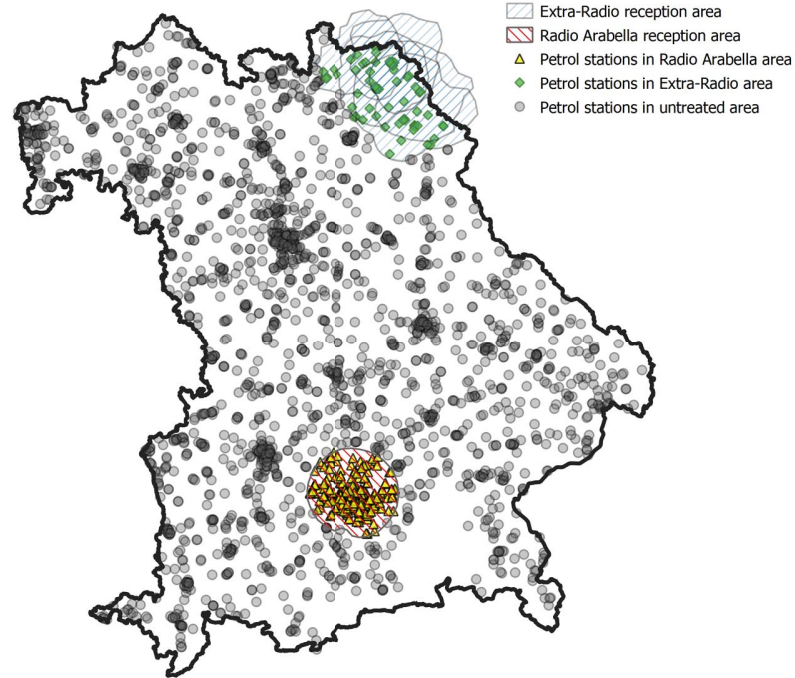
**Figure D.8:** Distribution of petrol stations across France



Note: The Figure shows the geographic distribution of petrol stations in France.

#### D.5 Local Radio Reports of Petrol Prices

There are 381 radio stations in Germany broadcasting via short-wave out of which 83 are active in Bavaria. Among these, we identified 60 radio stations that could potentially broadcast petrol prices, which we contacted. Among these stations, we identified four local radio stations that broadcasted local petrol prices (e.g. the three lowest price petrol stations in their reception area) more than once a day at some point after the MTU introduction in 2013 and 2014 and know the exact period of time of these broadcasts. We discard two of the radio stations because they already broadcasted the lowest petrol prices amongst those called in by their listeners before

**Figure D.9:** Radio reception areas and petrol stations in Bavaria

the MTU was introduced. The two remaining radio stations are *Radio Arabella*, which started its broadcast on 25 April 2014 and *Extra-Radio*, which started its broadcasts on 2 February 2014. We merge this information with data on the geographic availability of radio stations which we received from *fmlist.org*. Figure D.9 shows the reception areas of *Radio Arabella* and *Extra-Radio*. For each petrol station we can therefore say whether, on a particular day, it is within the reception area of a radio station broadcasting prices or not.

## D.6 Consumer Search and Information

The commuter data from the German Federal Employment Agency includes information on how many workers live in one municipality and work in another municipality for all municipality pairs for which there are more than 3 commuters as at the 30<sup>th</sup> June 2013. There were 11,197 municipalities in Germany in 2013. These are the smallest administrative unit in Germany, with a median size of  $19\text{km}^2$ . The centroid of a municipality is therefore a particularly good approximation of the home and work

address outside of the largest cities.<sup>52</sup> In the following, we detail our methodology of how to compute the share of perfectly informed potential customers of a petrol station. This description broadly follows the methodology introduced by Pennerstorfer et al. (2019).

To identify whether a particular commuter drives by a petrol station, we calculate the shortest path via the road network between the centroid of the home and work municipalities using the *osrmtime* package for Stata, written by Huber and Rust (2016). We only keep commutes that are less than 200 kilometers, since commutes above this threshold are unlikely to be daily commuters.

In a next step, we identify the set of petrol stations that could potentially lie on a commuter route. This step is essential in order to make the problem computationally tractable as we have 111,520 centroid combinations with positive commuter streams and around 14,700 petrol stations.

Firstly, using the coordinates of the two centroids of a commuting route, we identify the most Western and most Eastern longitude, as well as the most Southern and most Northern latitude of the centroids. Then, we push these boundaries 55 kilometers further out and draw a rectangle that contains both centroids. Using the coordinates of the petrol stations, we identify which petrol stations lie within the rectangle of a particular commuter route and could thus potentially be on the commuter route.

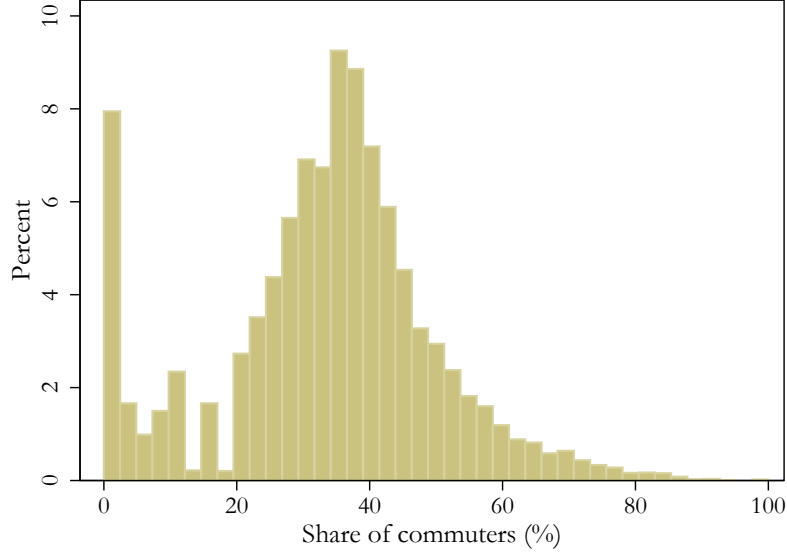
The set of potential petrol stations within the rectangle of a commuting route can still be large. In a second step, we therefore want to reduce this set by identifying the set of stations that are closer to the linear distance line between the two centroids than the difference between the shortest path via the road network and the linear distance, plus an additional buffer of 2 kilometers. This leaves us with a subset of petrol stations within the rectangle, which could potentially lie on the commuting route.

Finally, for the subset of stations that could potentially lie on the commuting route, we calculate the driving distance from the origin municipality to a petrol station and from the petrol station to the destination municipality. If the sum of these two routes is less than 250 meters longer than the shortest driving distance between origin and destination municipality, we consider this petrol station to be on the commuting route. We then consider that the number of commuters on this commuting route therefore all drive past the respective petrol station.

We assume that out-of-municipality commuters are perfectly informed about station prices in their home and work municipalities, as well as stations outside of these municipalities, that are on their daily way to work. The number of informed consumers of a petrol station  $i$ , denoted by  $I_i$ , are hence all out-of-municipality commuters residing

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<sup>52</sup> Large cities, such as Berlin or Munich, are treated as only one municipality.

**Figure D.10:** Distribution of ex ante informed consumer share across stations

Notes: The figure shows the distribution of petrol stations by the ex ante share of informed consumers. This is measured as the share of out-of-municipality commuters on 30 June 2013 as a share of all potential customers of a petrol station. We exclude petrol stations in municipalities with more than 1 million inhabitants in 2013 (Berlin, Hamburg, Munich and Cologne).

or working in the municipality of the station, as well as all driving past the station on their daily commute. The number of uninformed consumers,  $U_i$ , is proxied by employed individuals who do not commute outside their municipality. The share of ex ante informed consumers among potential customers of a petrol station is thus:

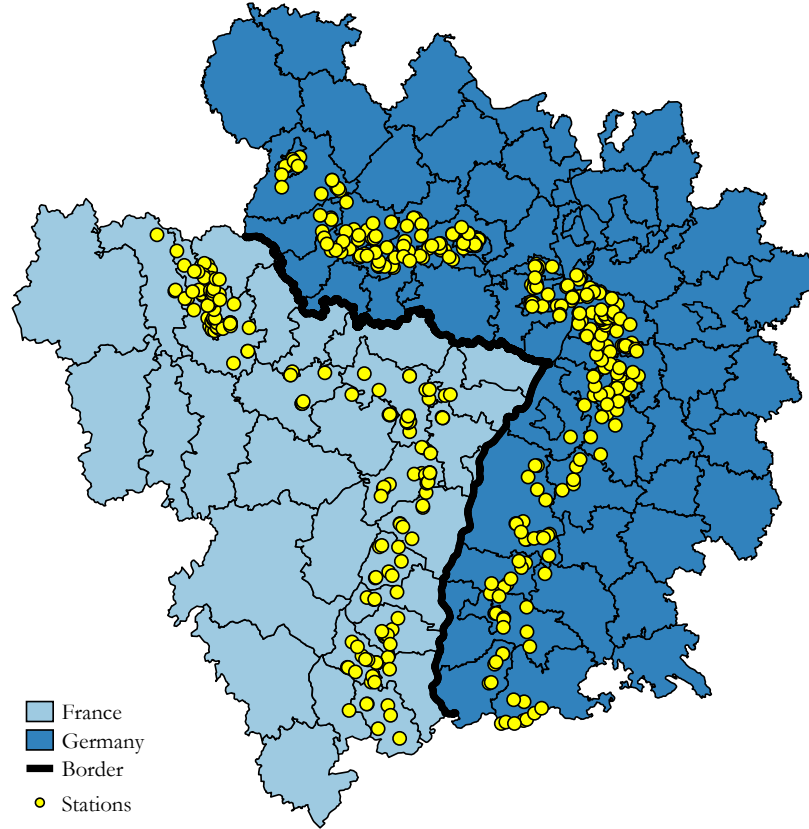
$$\zeta_i = \frac{I_i}{I_i + U_i}. \quad (4.33)$$

Figure D.10 shows the distribution of the ex ante share of informed consumers among potential customers of a petrol station. The majority of petrol stations have a share of ex ante informed consumers of between 20 percent and 60 percent.

## D.7 Donut Regression

Figure D.11 illustrates the identification strategy for the Donut-DiD regression analysis graphically. Petrol stations that are less than 20 kilometers away from the Franco-German border are not considered, because these could be in direct competition to each other and so spillovers of the treatment effect could occur. This would threaten the stable unit treatment value assumption. Petrol stations more than 40 kilometers away from the border could be subject to very different market conditions and are thus



**Figure D.11:** Petrol stations 20 to 40 kilometers from the Franco-German border

Notes: The thick, solid line represents the Franco-German border. Each point on the right of the border represents a petrol station in Germany, which is 20 to 40 kilometers away from the border. Each point on the left side of the border represents a petrol station in France, which is 20 to 40 kilometers away from the border. These are the petrol stations considered in our baseline Donut-DiD regression analysis.

also not considered. Each point in Figure D.11 thus represents a petrol station, either in France or in Germany, which is 20 to 40 kilometers away from the border.

In Table D.2, we re-estimate the Donut-DiD regression for the analysis period 12 April 2013 until 30 September 2014 using different distances to the Franco-German border. We find that the results are robust to changing the distances and the average effect of the MTU introduction is always around 1 Eurocent.

**Table D.2:** The effect of the MTU on retail margins using alternative donuts

	(1)	(2)	(3)	(4)	(5)	(6)
MTU introduction	-1.25*** (0.13)	-1.18*** (0.18)	-1.00*** (0.18)	-0.94*** (0.15)	-0.95*** (0.11)	-1.03*** (0.09)
Date fixed effects	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Station fixed effects	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Observations	206,140	111,319	166,595	234,831	416,647	551,184
Adjusted $R^2$	0.903	0.904	0.893	0.885	0.871	0.872
Mean retail margin	10.41	10.42	10.16	9.78	9.04	8.85

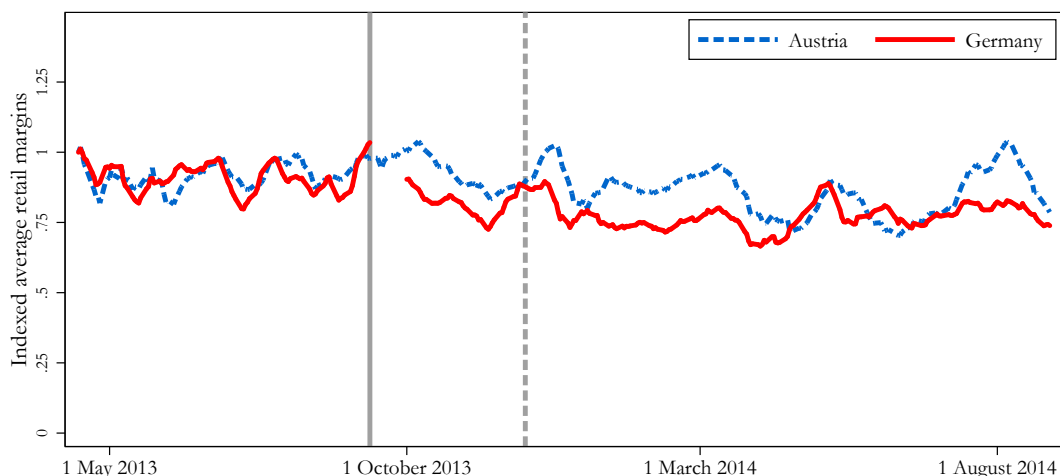
Notes: The test phase of the MTU (12 September 2013 until 30 November 2013) is dropped in all specifications. The analysis period is the 12 April 2013 until 30 September 2014 in all specifications. Column (1) uses only data for stations 10 to 40 km away from the Franco-German border. Column (2) uses only data for stations 20 to 40 km away from the Franco-German border. Column (3) uses only data for stations 20 to 50 km away from the Franco-German border. Column (4) uses only data for stations 20 to 60 km away from the Franco-German border. Column (5) uses only data for stations 20 to 80 km away from the Franco-German border. Column (6) uses only data for stations 20 to 100 km away from the Franco-German border.

Standard errors, clustered at the petrol station level, are in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

## D.8 Austria as a Control Group

To test the robustness of our estimates of the average effect of the MTU on retail margins in Germany, we use an alternative specification where we compare the indexed evolution of retail margins in Germany and Austria. Since we only have daily average prices and retail margins at the country level for Austria, we aggregate the German data to the same level and analyze the evolution of margins descriptively. We begin by estimating a daily average price for Germany, by taking the simple average of prices at each petrol station at 9 am, 12 pm and 5 pm and then averaging across petrol stations. Each observation is hence the daily simple average retail margin in the respective country. Since for Germany we overweight times that are less treated by the MTU, we underestimate the magnitude of the treatment effect. Figure D.12 shows the development of retail margins in Germany and Austria between April 2013 and August 2014. Although retail margins follow a common trend before the introduction of the MTU and are sometimes higher in Austria, sometimes higher in Germany, shortly after the increase in transparency in Germany, we observe a divergence in margins between the two groups and retail margins in Germany fall compared to Austria.

**Figure D.12:** Control: Austria

## D.9 Local Monopolists as a Control Group

Driving to another petrol station is costly and hence retail petrol markets are usually segmented geographically. We define local markets as driving distance catchment areas around a focal station. We assume that stations that do not face competition from another station in their catchment area act as local monopolists. Like in the analysis of Albæk et al. (1997) for the cement industry, these local monopolists are unaffected by increasing transparency and can therefore serve as a control group.

In Table D.3, we report the results of an estimation strategy in which we analyze the effect of the MTU on retail margins of petrol stations in Germany. We compare petrol stations in Germany, which have at least one competing petrol station in their catchment area to petrol stations that are local monopolists. Only petrol stations that are of a different brand are considered as competitors. Whereas we consider local monopolists as untreated by the introduction of the MTU, because consumers have no alternative in the vicinity and can thus not act upon the new information, stations that have a competitor in their market are considered treated. In Column (1), we define a local monopolist as not having any other station within a 1 kilometers radius. We find a treatment effect of 0.1 Eurocent, however, according to this definition 64% of petrol stations in Germany are local monopolists. We thus consider broader markets in Columns (2) and (3). In Column (2), we define local monopolists as not having a competing station within a 3 kilometers radius. We drop all petrol stations with a competitor within a 3 kilometers radius, but without a competitor within a 1 kilometers radius from the control group, as these are local monopolists according to the market

**Table D.3:** The effect of the MTU on retail margins using local monopolies

	(1)	(2)	(3)
MTU introduction	-0.08*** (0.03)	-0.18*** (0.03)	-0.23*** (0.04)
Date fixed effects	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Station fixed effects	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Observations	3,964,599	2,417,369	1,978,672
Share local monopolists	64.2%	42.2%	29.4%
Adjusted $R^2$	0.542	0.558	0.557
Mean retail margin	6.85	6.91	6.88

Notes: The test phase of the MTU (12 September 2013 until 30 November 2013) is dropped in all specifications. The analysis period is the 12 April 2013 until 30 September 2014 in all specifications. Column (1) compares petrol stations without competition within a 1 km radius to all other petrol stations. Column (2) compares petrol stations without competition within a 3 km radius to petrol stations with competition within a 1 km radius. Column (3) compares petrol stations without competition within a 5 km radius to petrol stations with competition within a 1 km radius. Only petrol stations that are of another brand are treated as competitors. Standard errors, clustered at the petrol station level, are in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

definition in Column (1). We find a treatment effect of 0.2 Eurocent per liter using 3 kilometers catchment areas. In Column (3), we repeat this analysis for 5 kilometers catchment area and find a similar treatment effect to Column (2). Overall, our results are consistent with Lemus and Luco (2019), who find that mandatory price disclosure reduced the time to reach a new equilibrium for oligopoly markets, but not for local monopolies.

Overall, the average effect of the MTU that we find using this specification is consistent with our estimates for the average effect of the MTU using France as a control group. We are likely to underestimate the treatment effect using the local monopolist identification strategy, since consumers in monopoly markets are likely also partially treated by the MTU. It therefore makes sense that the magnitude of the effect that we find using local monopolists is smaller than when comparing retail margins in Germany and France.

## D.10 Triple Difference Analysis

The DiD design in which we compare retail margins at petrol stations in Germany and France allows us to isolate the average treatment effect from shocks over time affecting

retail margins of petrol stations in Germany and France equally, as well as from shocks that are petrol station specific, but constant over time. However, there may still be transitory shocks unrelated to the MTU which affect petrol stations in Germany and France differently. Similarly, the DiD design in which we compare retail margins at petrol stations in Germany that are subject to local competition and petrol stations in Germany that are local monopolists allows us to isolate the average treatment effect from shocks over time affecting retail margins of local monopolists and competitive stations equally, as well as time-constant petrol station specific shocks. If there are transitory shocks which are unrelated to the MTU, but affect local monopolists differently to competitive stations, then this biases the estimates of the DiD estimator.

To address these concerns, we estimate a “triple difference” (DDD) regression. This compares the effect of the MTU on the difference in retail margins between local monopolists and competitive petrol stations in Germany to the difference in retail margins between local monopolists and competitive petrol stations in France. Comparing the DDD estimator to the DiD with France as a control group, the advantage is that we can isolate the average treatment effect from transitory shocks that affect local monopolists differently to competitive petrol stations, but that are constant across countries and thus unrelated to the MTU.

Specifically, we estimate the following fixed effects regression:

$$Y_{it} = \beta_0 + \beta_1(Post_t \times Germany_i \times Competitive_i) + \beta_2(Post_t \times Competitive_i) + \beta_3(Post_t \times Germany_i) + \mu_i + \gamma_t + \epsilon_{it} \quad (4.34)$$

where  $Y_{it}$  corresponds to the retail margin of station  $i$  at time  $t$  and  $Post_t \times Germany_i \times Competitive_i$  is a dummy equal to one, if an observation belongs to a petrol station  $i$  located in Germany, the observation is made at a date  $t$  after the 1 October 2013 and is a competitive station (i.e. not a local monopolist).  $(Post_t \times Competitive_i)$  is an interaction term equal to one, if an observation is of a competitive station after the introduction of the MTU.  $(Post_t \times Germany_i)$  is an interaction term equal to one, if an observation is of a petrol station in Germany after the introduction of the MTU.  $\mu_i$  are petrol station fixed effects, and  $\gamma_t$  are date fixed effects.

The estimated treatment effects of the triple difference strategy are reported in Table D.4. Column (1) shows that the MTU led to a decrease in retail margins of 0.5 Eurocent if we include data until the 30 September 2014. Column (2) shows that the treatment effect increases to 0.6 Eurocent if we include data until the 31 December 2014. The estimated treatment effects should be considered as a lower-bound of the true effect of the MTU, since there are likely to be spillovers between local monopolists and

**Table D.4:** The effect of the MTU on retail margins using a triple difference strategy

	(1)	(2)
MTU $\times$ competitive	-0.50*** (0.06)	-0.62*** (0.07)
Date fixed effects	<i>Yes</i>	<i>Yes</i>
Station-hour fixed effects	<i>Yes</i>	<i>Yes</i>
Observations	6,219,476	7,573,649
Adjusted $R^2$	0.921	0.905
Mean retail margin	10.90	11.15

Notes: The test phase of the MTU (12 September 2013 until 30 November 2013) is dropped in all specifications. We compare the difference between the change in retail margins before and after the MTU of competitive petrol stations and local monopolists in Germany to the difference between the change in retail margins before and after the MTU of competitive petrol stations and local monopolists in France. Column (1) uses data until 30 September 2014. Column (2) uses data until 31 December 2014.

Standard errors, clustered at the petrol station level, are in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

competitive petrol stations and thus local monopolists in Germany are also partially affected by the MTU.

### D.11 Sub-group Analysis

In Table D.5 we estimate the effect of the MTU on different sub-groups of petrol stations in Germany. Each sub-group is compared to all petrol stations in France between 12 April 2013 and 30 September 2014.<sup>53</sup> In Column (1), we estimate the effect of the MTU on Aral stations, which is the largest brand in Germany. We find that retail margins for Aral decreased by 0.33 Eurocent. In Column (2), we repeat the analysis for Shell, which is the second largest brand and often the most expensive. We find that retail margins decreased by 2.1 Eurocent. In Columns (3) and (4), we estimate the effect of the MTU on integrated brands and non-integrated brands separately. We find that the effect of the MTU is stronger on petrol stations of integrated brands than of non-integrated brands. Finally, in Column (5) we estimate the effect of the MTU only keeping petrol stations in Germany which are present in the sample for at least 100

<sup>53</sup> We do not observe brands in the French data and thus cannot construct brand-level control groups in France.

**Table D.5:** The effect of the MTU on retail margins by sub-group

	(1)	(2)	(3)	(4)	(5)
MTU introduction	-0.33*** (0.03)	-2.10*** (0.04)	-0.93*** (0.02)	-0.83*** (0.03)	-0.90*** (0.02)
Date fixed effects	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Station fixed effects	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Observations	2,999,649	2,827,722	4,647,714	3,826,639	5,697,926
Adjusted $R^2$	0.936	0.935	0.929	0.942	0.924
Mean retail margin	15.46	15.95	12.45	13.22	11.29

Notes: The test phase of the MTU (12 September 2013 until 30 November 2013) is dropped in all specifications. The analysis period is the 12 April 2013 until 30 September 2014 in all specifications. The control group in each specification are all petrol stations in France. Column (1) only uses Aral petrol stations in Germany. Column (2) only uses Shell petrol stations in Germany. Column (3) only uses integrated petrol stations in Germany. Column (4) only uses non-integrated petrol stations in Germany. Column (5) only uses petrol stations present for at least 100 days in our sample between 12 April 2013 and 12 September 2013 in Germany.

Standard errors, clustered at the petrol station level, are in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

days between 12 April 2013 and 12 September 2013. We find that the MTU decreased retail margins by 0.90 Eurocent for this sub-group compared to 0.89 Eurocent for all German stations in our sample found in Table 4.2. The average treatment effect in our main estimation results is thus not driven by differences in the sample composition before and after the MTU introduction.

## D.12 Conley Spatial HAC Standard Errors

Correlation between retail margins of petrol stations across space could lead to incorrect estimates of the standard errors. We therefore re-estimate the effect of the MTU on retail margins accounting for spatial dependence as proposed by Conley (1999).<sup>54</sup> Spatial autocorrelation is assumed to linearly decrease in distance up to a cut-off of 5 kilometers, which is in line with our definition of local markets. Temporal autocorrelation is assumed to decrease linearly in time up to a cut-off of 10 days.

Table D.6 shows that standard errors are smaller than in Table 4.2 and hence our main results are robust to using Conley spatial HAC standard errors.

<sup>54</sup>We implement this using the *reg2hdfe* package proposed by Fetzer (2014), which is based on an algorithm developed by Hsiang (2010).

**Table D.6:** The effect of the MTU on retail margins using Conley spatial HAC standard errors

	(1)	(2)	(3)	(4)	(5)	(6)
MTU introduction	-0.89*** (0.01)	-1.63*** (0.01)	-1.18*** (0.06)	-1.88*** (0.08)	-0.97*** (0.01)	-1.74*** (0.01)
Date fixed effects	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Station fixed effects	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Observations	6,219,476	7,573,649	111,319	136,561	5,196,002	6,323,512
Adjusted $R^2$	0.921	0.904	0.904	0.886	0.919	0.902
Mean retail margin	10.90	11.15	10.42	10.71	10.56	10.80

Notes: The test phase of the MTU (12 September 2013 until 30 November 2013) is dropped in all specifications. Column (1) uses data for all stations in Germany and France until 30 September 2014. Column (2) uses data for all stations in Germany and France until 31 December 2014. Column (3) uses only data for stations 20 to 40 km away from the Franco-German border until 30 September 2014. Column (4) uses only data for stations 20 to 40 km away from the Franco-German border until 31 December 2014. Column (5) uses data for all petrol stations except those without a competitor within a 5 km catchment area in Germany and France until 30 September 2014. Column (6) uses data for all petrol stations except those without a competitor within a 5 km catchment area in Germany and France until 31 December 2014.

Standard errors clustered are estimated using Conley spatial HAC standard errors with a spatial cut-off of 5 km and a time cut-off of 10 days and are in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

### D.13 Dropping September 2013

To show that the estimated average treatment effect of the MTU is not driven by the margin increase in Germany in the first half of September 2013, we re-estimate all specifications presented in Table 4.2 dropping September 2013. The results are presented in Table D.7 and show that the average treatment effect of the MTU is robust to dropping September 2013.



**Table D.7:** The effect of the MTU on retail margins omitting September 2013

	(1)	(2)	(3)	(4)	(5)	(6)
MTU introduction	-0.67*** (0.02)	-1.41*** (0.03)	-0.95*** (0.18)	-1.65*** (0.23)	-0.76*** (0.02)	-1.52*** (0.03)
Date fixed effects	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Station fixed effects	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Observations	6,050,265	7,404,438	108,371	133,613	5,053,680	6,181,190
Adjusted $R^2$	0.925	0.907	0.908	0.889	0.923	0.905
Mean retail margin	10.88	11.14	10.39	10.69	10.53	10.79

Notes: The test phase of the MTU (12 September 2013 until 30 November 2013) and the margin increase in September 2013 are dropped in all specifications. Column (1) uses data for all stations in Germany and France until 30 September 2014. Column (2) uses data for all stations in Germany and France until 31 December 2014. Column (3) uses only data for stations 20 to 40 km away from the Franco-German border until 30 September 2014. Column (4) uses only data for stations 20 to 40 km away from the Franco-German border until 31 December 2014. Column (5) uses data for all petrol stations except those without a competitor within a 5 km catchment area in Germany and France until 30 September 2014. Column (6) uses data for all petrol stations except those without a competitor within a 5 km catchment area in Germany and France until 31 December 2014.

Standard errors, clustered at the petrol station level, are in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$



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